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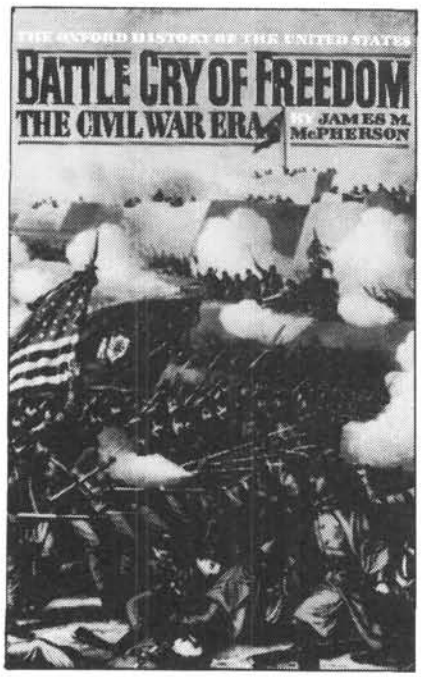
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In This Issue

In addition to review essays on recent work in black history and public history, this issue contains articles on medieval and Renaissance Europe, Eurasia and the Mideast.

Kenneth R. Stow uses martyr lists to construct a picture of the urban Jewish family of the Rhineland in the high Middle Ages. The martyr lists, which record the deaths of Jews during massacres of the First Crusades and later, supplement the little that is known about urban family structure from that time. Stow finds that the nuclear family predominated and averaged about 3.5 persons. Women were active in economic affairs, and many were educated. Intimate relations in marriage exhibited characteristics of the bourgeois family model of modern Europe, according to Stow. He also links the families he studies with Jewish families in southern Italy of an earlier time and notes the emergence of a different type of Jewish family in Eastern Europe at a later time.

William H. McNeill restores perspective to our understanding of transport within the Eurasian ecumene by reminding us of the important role played by non-wheeled transport. From the third to the thirteenth centuries, caravans of camels carried goods long distances about as efficiently as did ships, since camels tapped free energy by casual grazing just as ships caught wind with their sails. Improvements in ship design eventually made ships the more reliable and efficient carriers of bulk cargoes. The restricted carrying capacity of camels rendered caravans less competitive and was an important factor, McNeill contends, in the failure of the Moslem heartland to develop as rapidly as China or Europe.

Those who study past letters of recommendation—and probably, too, those of us who write and receive such letters today—will look at them with different eyes after reading the article on Renaissance patronage by **Vincent Ilardi**. He has discovered that, in the mid-fifteenth century, many letters of recommendation were coded with a system of secret signs designed to tell the receiver whether the recommendation was to be disregarded or accepted as an authentic expression of support. Ilardi's evidence as yet includes only letters from Duke Francesco Sforza of Milan and Cardinal Guillaume d'Estouteville of Rouen, but he believes that other leading patrons, faced with the problem of a great demand for letters of recommendation, most likely adopted similar practices to distinguish the value of their many expressions of support.

Robert L. Harris, Jr. reviews four recent works that reveal the growth and acceptance of Afro-American history by professional historians and the public. Although pioneers such as George Washington Williams, W. E. B. Du Bois, and Carter G. Woodson laid the foundation for professional respect of Afro-American history, the field remained for almost a century on the periphery of professional historical scholarship. After World War II, changes in American society and in scholarly method and interest led to the emergence of Afro-American history as a major field of study and to the appearance of works by black scholars in the leading publications of the profession. Harris emphasizes the need to nurture Afro-American history in all its dimensions.

Public history is another field that has grown rapidly in recent years. **W. Andrew Achenbaum** surveys the variety of research and projects of historical reconstruction being developed in this field. He focuses in particular on two anthologies that are devoted to defining the major challenges and opportunities for public or "applied" historians. Among other things, Achenbaum observes that public historians are involved in a wide range of activities and have not yet achieved a consensus concerning the proper objects of study and the ultimate purposes of this branch of historical work.

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Contributors

W. Andrew Achenbaum is a professor of history at the University of Michigan, where he worked on his Ph.D. with Robert Berkhofer, Wilbur Cohen, Kenneth Lockridge, and Maris Vinovskis. He is also director of the Aging Society Policy Studies Center in the University of Michigan's Institute of Gerontology. Achenbaum has written extensively about the changing meanings and experiences of being old in the United States. His latest book is *Social Security: Visions and Revisions* (1986). He is currently investigating the emergence of gerontology as a scholarly enterprise.

Robert L. Harris, Jr., is associate professor of Afro-American history and director of the Africana Studies and Research Center at Cornell University. He has published most recently *Teaching Afro-American History* (1985), in the AHA pamphlet series. He is interested in Afro-American historiography as well as black thought and culture. A graduate of Roosevelt University, he received his doctorate in 1974 from Northwestern University, where he studied under George Fredrickson, now at Stanford University, Sterling Stuckey, and Robert Wiebe. He is completing a book-length manuscript on the influence of the United Nations on the Afro-American struggle for freedom and equality.

Vincent Ilardi, professor of history at the University of Massachusetts (Amherst), received his B.A. from Rutgers University (Newark) and his Ph.D. from Harvard. A specialist in Italian Renaissance political and diplomatic history, he has co-edited the first two volumes of *Dispatches of the Milanese Ambassadors in France and Burgundy, 1450–1483* (1970–71) and edited the third volume (1981). Since 1971, he has also served as general editor of this series. His other major publications include *Occhiali alla Corte di Francesco e Galeazzo Maria Sforza* (Milan, 1978) and a collection of articles, *Studies in Italian Renaissance Diplomatic History* (1986). Recently, he has endeavored to alert the profession to the great variety of information to be found in diplomatic documents by publishing articles on the history of optics, cattle husbandry in Lombardy, and the present one on coded letters of recommendation. He is now preparing a guide to early Renaissance diplomatic institutions, archival collections, and his own microfilm collection of about two million documents.

William H. McNeill is a professor emeritus at the University of Chicago and former president of the AHA. In his examination of world history, he has published some twenty-three books, including *The Rise of the West: A History of the Human Community* (1963), *Plagues and Peoples* (1976), and *Pursuit of Power* (1982). His forthcoming book, *The Mystery of Man: A Biography of Arnold J. Toynbee*, is scheduled for publication on the centenary of Toynbee's birth, April, 1989.

Kenneth R. Stow is a professor of Jewish history at the University of Haifa. He studied history at Columbia University with Gerson Cohen, John Mundy, and Robert Somerville. His works include *Catholic Thought and Papal Jewry Policy 1555–1593* (1977), *Taxation, Community and State: The Jews and the Fiscal Foundations of the Early Modern Papal State* (1982), and *The "1007 Anonymous" and Papal Sovereignty: Jewish Perceptions of the Papacy and Papal Policy in the High Middle Ages* (1984). Currently, he is studying the effects of papal policy on Jewish internal life, as in his recent article, "Donne ebree a Roma nell'età del ghetto: Affetto, dipendenza, autonomia" (*Rassegna Mensile di Israel*, 1986).

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The Jewish Family in the Rhineland in the High Middle Ages: Form and Function

KENNETH R. STOW

A RARE GLIMPSE INTO THE INNER WORKINGS of the medieval Jewish family is offered by Eleazar ben Judah of Worms, better known as the Rokeah, in a dirge he composed following the brutal murder of his wife and two daughters and the wounding of his only son in the city of Worms in the year 1197.[1] A song of praise to the "stalwart wife,"[2] replete with stereotypes, the poem is an unsurpassed portrait of an ideal Jewish family unit. "Ideal" is to be understood in more than one sense. If Dulcia, the wife of the Rokeah, did half of what he ascribed to her, she must have been a remarkable individual.

Immediately before her murder, Dulcia had purchased parchment to make books, probably account books, for it was she who took charge of family economics and freed her husband to pursue his studies and teaching.[3] Dulcia was also responsible for such traditionally female activities as tending the house and rearing the children, and her "motherly" pursuits extended to caring for the needs of the students in the yeshiva headed by her husband. In addition, she taught laws (*halakhot*) and ritual observances to the women of the community, and her attendance at synagogue services was constant, although this was not required of women. Beyond these things, she labored at stitching parchments together in preparation for the writing of Torah scrolls, sewed dresses for brides, and provided food for weddings. She even undertook the honored task of washing the dead and dressing them in shrouds before interment. Dulcia evoked strong feelings and emotions from the Rokeah, who wrote of his "desolation and suffering" at her death.

My tender [literally, righteous] wife . . . How it troubles me about her. How much blood was spilled; and there she lay before me dying . . . Her husband's heart trusted in her; she

I would like to thank Ben Zion Kedar, Hermann van der Wee, Sergio della Pergola, and Esther Cohen for reading an earlier version of this article, Ehud Makov for statistical advice, and especially Robert Cohen for teaching me the rudiments of writing family history. Unless otherwise noted, all translations are mine.

[1] The text may be conveniently found in A. M. Haberman, *Sefer Gezerot 'Ashkenaz ve-Zarfat* (Jerusalem, 1971), 164–67.

[2] For this genre, see *Encyclopaedia Judaica* (Jerusalem, 1971), s.v. "Eshet Hayyil," 6: 886. On the dirge, see Ivan G. Marcus, "Mothers, Martyrs, and Moneymakers: Some Jewish Women in Medieval Europe," *Conservative Judaism*, 38 (1986): 34–45.

[3] For the eventual institutionalization of such activity in the "scholar's family," see Emanuel Etkes, "Family and Study of Torah in Lithuanian Talmudist Circles in the Nineteenth Century" (Hebrew), *Zion*, 51 (1986): 87–106.

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returned [his kindness] with goodness . . . On the day of the Sabbath, she would sit giving explanations [of the weekly Torah portion] and her husband listened attentively . . . She was glad to do the will of her husband and never once was she angry [with him]; she was pleasant in her ways.[4]

Less is said in the poem about Bellette and Hannah, the daughters, but it is clear that their education included reading and writing and was not confined to housework and singing.[5] They too were dear to the author, who lauds their accomplishments with pride. His son, Jacob, was not eulogized, most probably because he survived the attack.

The family unit of the Rokeah was, then, close-knit and small, comprising only five souls. It was two generational; there is no indication that grandparents were present in the household. This was not an oversight. Had members of a third generation been present, the Rokeah would have made some reference to their fate during the attack. At least one grandparent was probably still alive; the Rokeah was only about thirty to thirty-six years old at the time of the attack. As for the individual family members, each of them was skilled and educated, and all were bound together by close emotional ties. The lines dividing the sexes also tended to blur, at least on the economic plane, although this lack of differentiation of sex roles was not complete. The Rokeah ran the yeshiva; his wife taught women only. Nevertheless, both taught, and both taught the same subject, liturgy and laws (*halachah*), if, no doubt, on different levels of sophistication. The family of the Rokeah appears best characterized, therefore, as a two-generational, partially non-role-differentiated, affective nuclear family. Seemingly more appropriate for a time much later than the twelfth century, this definition requires substantiation. Was the family of the Rokeah's dirge unique, or did it represent the norm? And if the norm, did this norm apply to Jewish families only? The answers to these questions have broad implications that go beyond the substantiation of the above characterization. They contain basic information on the nature of medieval Jewish life and culture.

IMPORTANT HINTS ABOUT THE JEWISH FAMILY IN EUROPE come from the southern Italian chronicle and genealogy of Ahimaaz ben Paltiel in 1054, which vividly portrays incidents in the lives of parents and children and describes small conjugal units much like the family of the Rokeah.[6] A number of abbreviated rabbinic genealogies[7] and a substantial corpus of legal texts contribute further informa-

[4] Haberman, *Sefer Gezerot*, 165. On the possible representativeness of Dulcia's education, see Nathan Morris, *A History of Jewish Education from the Earliest Times to the Sixteenth Century* (Hebrew), 3 vols. (Jerusalem, 1977), 1: 317–18, 321.

[5] On women's names, see Luigi Chiappelli, *La Donna pistoiese del tempo antico* (Pistoia, 1914), 84–93. Jewish and Christian women's names were frequently of Latin origin.

[6] Benjamin Klar, *The Scroll of Ahimaaz* (Hebrew) (Jerusalem, 1974).

[7] For previous studies on ritual and legal aspects of marriage, see A. H. Freimann, *Seder Kiddushin ve-Nissuin 'aharei Hatimat ha-Talmud* [Betrothal and marital practices in the post-Talmudic period] (Jerusalem, 1945); Z. W. Falk, *Jewish Matrimonial Law in the Middle Ages* (Oxford, 1966); and L. M.

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tion.[8] No detailed portrait of Jewish family structure would be possible, however, were it not for a memorial list of Jewish martyrs who perished in the Rhineland city of Mainz during the massacres of the First Crusade in the spring of 1096. Composed in the generations following the massacres, the list is arranged strictly according to family units, and, significantly, its contents may be compared with those of similar, though briefer, contemporary lists.[9] The data from these memorial lists are verifiable, especially in the case of Mainz. Nearly all of the martyrs who belonged to prominent Rhineland rabbinical families were also named in other chronicles of the First Crusade, like that of Solomon ben Samson.[10] These chronicles stress that people died—in fact, ritually took their own lives—as families: men systematically and sacrificially slaughtered their wives and children. In addition, the chronicles indicate that, hoping for safety, the Jews of Mainz entered the keep of the local bishop a full day before the attack, so that when the assault came, families were physically united.

The Mainz memorial list is also comprehensive. It refers to nearly 500 individuals, a large majority of the city's Jews, who were nearly equally divided between men and women.[11] The sex ratio for a total of 120 adult males and 124 adult females was 97. Alternately, for 94 married (or once-married) males and 104 such women, it was 90. These ratios are close enough to the statistical norm of 100 males to 105 females to signify comprehensiveness.[12] Regrettably, the source allows no way of calculating a sex ratio for children. The list all too often refers to them as "children" (*banim*) without designating gender. Information is never provided about ages.[13]

Epstein, *Marriage Laws in the Bible and the Talmud* (Cambridge, 1942). See, too, Harry Schnur, "Judische ehe und familie im Mittelalter," *Love and Marriage in the Twelfth Century*, Willy van Hoecke and Andries Welkenhuysen, eds. (Louvain, 1981), 88–101; D. M. Feldman, *Marital Relations, Birth Control and Abortion in Jewish Law* (New York, 1974). For late ancient families, see Zev Safrai, "Mivne ha-Mishpaha be-Tequfat ha-Mishnah ve-ha-Talmud" [Family structure in Mishnaic and Talmudic times], *Milet: Mehkarim be-Toledot Yisrael u-be-Tarbuto*, 1 (1982): 129–56; and for the early modern period, the pioneering study of Jacob Katz, *Tradition and Crisis* (New York, 1971), chaps. 14 and 15.

[8] We are much better informed for the Jews under Islam. See, for example, M. A. Friedman, *Jewish Marriage in Palestine, A Cairo Geniza Study*, 2 vols. (New York and Tel Aviv, 1980–81); and especially S. D. Goitein, *A Mediterranean Society*, vol. 3, *The Family* (Berkeley, Calif., 1978).

[9] See Sigmund Salfeld, *Das Martyrologium des Nürnberger Memorbuches* (Berlin, 1898), 10–12, for the Mainz list (and *passim* for others). See its use by Bernhard Blumenkranz, "Germany," *The Dark Ages*, vol. 4, *The World History of the Jewish People*, C. Roth, ed. (Tel Aviv, 1966), 165; and Abraham Grossman, *The Early Sages of Ashkenaz* (Hebrew) (Jerusalem, 1981), 20.

[10] A readily available version appears in Haberman, *Sefer Gezerot*, 24–60.

[11] Solomon ben Samson gives the inflated figure of 1,100; see Haberman, *Sefer Gezerot*; 32, see also 26, 35, and 39.

[12] For this norm, see J. C. Russell, *Late Ancient and Medieval Population* (Philadelphia, Pa., 1958), 13–14; and the refinements in Roland Pressat, *Statistical Demography*, D. A. Courtney, trans. (London, 1978), 13–19.

[13] This is not surprising. Kenneth Wachter, "Age Pyramid Variances," *Statistical Studies of Historical Social Structure* (New York, 1978), 189, indicates that "only eight listings by household with ages are known for England before 1800." Zvi Avneri, "Medieval Jewish Epitaphs (on gravestones) from Magenza," *Mehkarim be-Toledot 'am-Yisrael ve-'Erets-Yisrael* [Studies in the history of the Jewish people and the land of Israel], 1 (1970): 141–67, contains only random evidence. Information on ages would have permitted many analytical refinements.

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The memorial list may be broken down into 168 distinct units. Of these, 122 are families with a total of 198 first-generation husbands and wives. In 77 families, there were both a husband and a wife, in 17 only a husband, and in 27 a wife alone. There was also a family of six orphans. Children were distributed as follows: 11 families had three or more children (2 had five, 1 four or more, 2 four, 1 three or more, 4 three, and 1 had the six orphans), and 34 had one child only. In the middle range, 16 families had two children, and 41 are just listed as having *banim*, which, as a plural, must mean two or more. Twenty families were childless, and this number (16.4 percent) lies well within the limits of the expected childless couples,[14] both statistically and in terms of contemporary medical technology.[15]

The remaining 46 units on the list were unmarried individuals (solitaries)—26 males and 20 females. The percentage of those currently or once married was thus at least 80. In fact, it may have been considerably higher.[16] Most of the 26 males were probably younger students in the yeshiva, and many of the 20 females were probably domestics not yet ready for marriage. Childless widows and widowers, calculated at the same rate as childless couples, numbered a maximum of 7 or 8.[17] There is no possibility of widows or widowers with living children, because all children on the list are recorded along with their parents. Nearly everybody eligible for marriage, therefore, was not only currently or once married but was also reported as such.[18]

The greatest difficulty presented by the memorial list is that of assessing the number of children in the 41 families listed as having *banim*. A chi-square analysis, using the less complete martyr lists from the cities of Worms and Cologne as a basis for comparison (see Tables 1.0 and 1.1) shows that the distribution of the children in the 79 families actually observed may be accepted as reflecting the distribution of children in the population as a whole. Hence it may be said that 26 (or 79 percent) of the 41 families had two children and 15 (or 21 percent) had three or more (7 three, 3 four, 3 five, and 2 six). Altogether, this makes 216 children, or an average of 1.77 per family.

Although a lack of data on the size of non-Jewish families prevents corroboration of these results with this other population, it is most helpful to compare the 122 families on the memorial list with the 10 families recorded in the chronicle of

[14] See Jack Goody, *The Development of the Family and Marriage in Europe* (Cambridge, 1983), 44. Some couples may have been young, however, and eventually would have had children.

[15] See M. J. Levy, Jr., and A. J. Coale, *et al.*, *Aspects of the Analysis of Family Structure* (Princeton, N.J., 1965), 49, 56, and 64.

[16] On the near universality of marriage among Jews, see Massimo Livi-Bacci, "Ebrei, aristocratici e cittadini: precursori del declino della fecondità," *Quaderni storici*, 54 (1983): 913–40, especially 916.

[17] In a parallel martyr list for Worms (Salfeld, *Das Martyrologium*, 8–9), of 12 women solitaries, 4 were childless widows and 3 older spinsters. Servants occasionally are distinguished by the term *benei beto* (household), as opposed to *banim* (children).

[18] Factors encouraging this high rate include the Jewish laws on succession; see the entries on "Succession," "Dowry," etc., in the *Encyclopaedia Judaica* and the thorough *Jewish Encyclopedia* (New York, 1905). There was also the absence of religious vocations; see John Eastburn Boswell, "*Expositio* and *Oblatio*: The Abandonment of Children and the Ancient and Medieval Family," *AHR*, 89 (1984): 10–33.

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Solomon ben Samson. As in the entire Jewish sample, 80 percent of the families in the chronicle had two or fewer children and 20 percent had three or more.[19] Possibly, there were older children in these families—as well as in other families—who were studying or traveling on business outside of Mainz at the time of the massacre and who were not captured in the sources. Possibly, too, more instances occurred than the one instance actually recorded, in which a son had created his own family and was listed separately from his parents. Nevertheless, the numbers of these cases were presumably too small to noticeably modify any conclusions. The correspondence between families in the chronicle and the memorial list is, in fact, remarkable. Studies of non-Jewish families in both the medieval and early modern periods have shown that the more settled, aristocratic, or monied a family, the greater the tendency to have a large number of children and a complex family structure.[20] The martyrs in Solomon ben Samson's chronicle, many of whose families had been living in the city of Mainz for as long as 200 years, came from precisely such well-entrenched households.[21] Consequently, the size of these families represents an upper limit. That they were preponderantly small supports the conclusions drawn about the Jewish community of Mainz as a whole. Indeed, the unexpected uniformity of family size throughout the community suggests that the Jewish society of the Rhineland was homogeneous.

Measured against the above standards, the family of the Rokeah, with three children, was larger than average. This circumstance raises questions that I do not deal with here but that should be noted as issues for further research. For example, some Jews living in the generations following the martyrdoms of the First Crusade were presumably eager to have more children to make up for losses, but this is not a certainty. Similiarly, there may be some cause for wondering if the figure of 1.77 children (or 2.12 children, if calculations are made solely on the basis of the 102 families that had children) suggests a demographic decline or stasis prior to 1096, in response to factors such as upward mobility and increased wealth in individual families.[22] Or this picture may have been no more than a reflection of the enormously high medieval mortality rates.[23]

Irrespective of specific causes, the fact remains that the size of the average Rhineland family at the time of the First Crusade was 3.77 persons (1.77 children

[19] The precise division of children was 1 family with four, 1 with three, 3 with two, 3 with one, and 2 with none.

[20] See J. L. Flandrin, *Families in Former Times: Kinship, Household and Sexuality*, R. Southern, trans. (Cambridge, 1976), 88–91; Diane Hughes, "Domestic Ideals and Social Behavior: Evidence from Medieval Genoa," *The Family in History*, C. E. Rosenberg, ed. (Philadelphia, Pa., 1975), 129; and David Herlihy, *Medieval Households* (Cambridge, 1985), 155.

[21] See Grossman, *The Early Sages*, 10–12.

[22] On possible relationships between contraception and upward mobility, see Livi-Bacci, "Ebrei, aristocratici," 936.

[23] Roberto Bachi observed (personal communication, 1985) that high medieval mortality rates should have barely enabled Jews to maintain numeric stability, let alone register increases. In Mainz families with one child only, the ratio of one-parent families to two-parent families was 19:15 in contrast to a ratio of 2:3 in families with two or more children. The family, therefore, was often dissolved before more than one child could be born.

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TABLE 1.0

Families in Mainz

No. of Children	*No. of Families*	*% of Families*	*Total Children*	*% of Children*
0	20	16.39	0	———
1	34	27.87	34	15.74
2	42	34.43	84	38.88
3	11	8.66	32	14.69
4	5	4.33	21	9.78
5	5	4.33	21	9.78
6	3	2.16	16	7.34
Eq. or > 3	1	.82	Eq. or > 3	1.85
Eq. or > 4	1	.82	Eq. or > 4	1.85
Total	122	99.81	216	99.91

No. of Families	122
No. of Children	216
No. of Adults	198
Average No. of Children (median = 2)	1.77
Average Family Size	3.39

Chi-Square 3.093, deg. freedom 6, probability = 0.7971
—Mainz compared with the martyrs of Solomon ben Samson's chronicle

Chi-Square 7.194, deg. freedom 5, probability > .10
—Mainz compared with the martyrs of Worms & Cologne

TABLE 1.1

Comparison of Families in Mainz, Worms, and Cologne

	Mainz		Worms & Cologne	
No. of Children	*Observed*	*Expected*	*Observed*	*Expected*
0	20	21.96	22	20.00
1	34	33.90	31	30.96
2	16	16.20	15	15.17
3	4	2.13	0	1.94
4	2	2.61	Eq. or > 3	2.38
Eq. or > 5	3	2.13	1	1.94
Total Observed	79	21.96	72	
Observation Eq. or > 2	43	16.20	44	
Total	122	2.13	116	

Chi-Square 7.194, deg. freedom 5, probability > .10
Chi-Square 1.855, deg. freedom 3, probability > .10
—when combining families equal or > 3 into one cell

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plus two parents)—or, more precisely, only 3.39 persons, since in 44 of the 122 families there was only one parent. This figure is very close to the average of 3.8 persons established by David Herlihy and Christiane Klapisch-Zuber for the Florentine family in 1427.[24] It also approximates the average of 1.8 children determined by Peter Laslett, and seconded by J. L. Flandrin, for the families of tradespeople in the early modern period.[25] More important, these averages are valid for Jewish families throughout the Rhineland. If the results of the chi-square test for Mainz are used to extrapolate from the less complete martyr lists for Worms and Cologne, the findings are remarkably similar: 83 percent of the families had two children or fewer (including 17.7 percent childless), and 17 percent had more than three (Table 2).

TABLE 2
Combined Mainz, Worms, Cologne Families

No. of Children	*No. of Families*	*% of Families*
0	42	17.7
1	65	27.3
2	92	38.8
3	12	5.0
4	15	6.3
5	12	5.0
Total	238	99.6

	Mainz	*Worms*	*Cologne*	*Total*
No. of Families	122	96	20	238
No. of Children	216	164	23	403
No. of Adults	198	152	35	385
Average No. of Children	1.77	1.74	1.15	1.69
(median)	2	2	1.5	
Average Family Size	3.39	3.29	2.9	3.31

[24] David Herlihy and Christiane Klapisch-Zuber, *Les Toscans et leurs familles: Une Étude du catasto florentin de 1427* (Paris, 1978), 166–67. A. J. Coale, "Estimates of the Average Size of Households," in Levy and Coale, *Aspects of the Analysis of Family Structure*, 68, argues that the average size of nuclear families in societies with high death rates ought to be 3.3 persons. See also Michael Mitterauer and Reinhard Sieder, *The European Family*, K. Oosterveen and M. Holzinger, trans. (Chicago, 1982), 27. For the very late Middle Ages, Herlihy, *Medieval Households*, 143–44, reports gradual increases up to nearly 6 persons on average. *Les Toscans*, 144–46, notes a small number of Jewish families in Florence with an average size of 9 to 10 persons. These, however, are banking families with a considerable number of assistants and servants, as is seen also in Michael Toch, "The Jewish Community of Nuremberg in the Year 1489, Social and Demographic Structure" (Hebrew), *Zion*, 45 (1980): 60–72, especially 65–66.

[25] Flandrin, *Families in Former Times*, 56; and Peter Laslett, *The World We Have Lost* (London, 1965), 64. The smallness of the Jewish family, in combination with the age (twenty to twenty-two) of the Rokeah and Dulcia on first becoming parents, contrasts with the thesis on the rise of small families in J. Hajnal, "European Marriage Patterns in Perspective," *Population in History*, D. V. Glass, ed. (London, 1965), 101–43.

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Family size also appears to have remained stable. Martyr lists from Frankfurt am Main from 1241 and numerous other German cities from 1298 show only a slight upward movement toward an average of 3.69 persons, with 71 percent of the families having two or fewer children (including 20 percent childless), and 29 percent three or more. This stability forms a contrast to the large increases in the general European population at this time. In addition, the average Jewish family was the type most frequently encountered.[26] Of the 122 families on the memorial list, 96 had two or fewer children. Contemporary Jews who did not calculate percentages but thought in terms of overall impressions would doubtlessly have believed that the normal family was truly small. Perhaps they also sensed that an inordinately large social space was occupied by a very high percentage of below-average family units: 48 of the 102 families with children consisted of only three individuals, that is, two children and one parent (14 families), or one child and two parents (34 families). This impression would have been reinforced by the paucity of extended (multigenerational or joint) family units. The memorial list records only 13 multigenerational families (10.5 percent of the total). No family had more than three generations.[27] Furthermore, with but one exception—a certain Bella, her daughter Rachel, and Rachel's two sons—third generations were recorded simply as "their children" (*u-beneihem*), with no information about number, sex, or age. Three-generational families appear simply as groupings of parents, children, and grandchildren. No other relations are mentioned. In only two instances does this pattern vary. In the first, a family consists of grandparents and grandchildren but no second generation. In the second, an upward extension of a conjugal unit includes a father, mother, and daughter, together with two sisters of the father and *his* mother. Since the father is the family head, there is some question whether this should be considered a three-generational unit at all.[28]

Joint families are even rarer. There are only eight of them, and not all properly fall into this category. Four are groupings of unmarried brothers and sisters,[29] and two more are identical with the multigenerational families just discussed. In the two truly joint families remaining, the first contains three, and the second two, first-generation brothers. Yet these were surely not the only instances out of the total 122 in which the heads of families had siblings. In other cases, however, the memorial list ignored the fact. The two exceptions thus suggest that the brothers

[26] On "frequency," see Flandrin, *Families in Former Times*, 70–74, and 90–91.

[27] Compare this with the figure of 15.5 percent for extended families among Rouen merchants in 1700, in Flandrin, *Families in Former Times*, 90.

[28] Mitterauer and Sieder, *The European Family*, 32 and 37, point to the rarity of families with living representatives from more than two generations; see, too, M. J. Levy, Jr., 50, and L. A. Fallers, 75, both in Levy and Coale, *Aspects of the Analysis of Family Structure*; and Safrai, "Mivne ha-Mishpaḥa," 150.

[29] These four were counted among the solitaries since there is no other familial indication for them. The list also records four families with second-generation brothers and grandchildren, but these should be considered at most extended families. Compare Safrai, "Mivne ha-Mishpaḥa," 149–52, on "fraternal partnerships" among ancient Jews.

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TABLE 3
German Cities, 1241 and 1298

No. of Children	*Frankfurt a.m.*		*Nürnberg*		*Würzberg*		*Bamberg*		*Neumarkt*		*Forchheim*	
	F*	% †	F*	% †	F*	% †	F*	% †	F*	% †	F*	% †
0	8	23.5	37	21.6	26	16.4	5	16.1	2	13.3	9	37.5
1	9	26.5	50	29.2	49	31.0	12	38.7	5	33.3	5	20.8
2	5	14.7	27	38.9	35	22.2	11	35.5	3	20.0	5	20.8
3	4	11.8	22	12.8	21	13.3	2	6.5	4	26.7	5	20.8
4	4	11.8	16	9.4	17	10.8	—	—	1	6.7	—	—
5	2	5.9	16	9.4	3	1.9	—	—	—	—	—	—
6	1	2.9	2	1.2	6	3.7	—	—	—	—	—	—
7	1	2.9	1	0.6	1	0.7	—	—	—	—	—	—
Total	34		171		158		30		15		24	
Adults	57		297		281		45		24		43	
Children	60		343		321		40		27		30	
Average No. of Children	1.77		2.0		2.03		1.33		1.8		1.25	
Average Family Size	3.40		3.75		3.81		2.83		3.4		3.04	

Cumulative	
Total Families	432
Total Adults	802
Total Children	791
Average Children per Family	1.83
Average Family Size	3.69

*F = Number of Families
† % = Percentage of Families

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had constituted themselves into a *frérèche* or some other type of consortial relationship, too distinctive and formalized to be ignored.[30] Out of 122 family units, therefore, only 15 may be classified as joint or three-generational in any way. No wonder the author of the memorial list was preoccupied with two-generational conjugal family units or that this preoccupation paralleled the emphasis of the Crusade chroniclers on husbands slaughtering wives and children.[31] Some Jews even seem to have looked askance at households composed of more than two generations, seeing in them a source of perpetual friction.[32]

Despite this focus on nuclear families, Jews did attach significance to larger family units. The Rokeah, a proud descendant of one of the five most illustrious Jewish families in the Rhineland, the Kalonymides, made a great point of recording the names of prominent (male) ancestors in the Rhineland, and previously in Italy, who had carried forward the chain of tradition on which his learning and piety were based.[33] A sense of family continuity that even memorializes significant women is conveyed by Ahimaaz ben Paltiel in a continuous record of his illustrious forebears in Apulia, beginning in the late eighth century. Ahimaaz's goal was to demonstrate how the two most important branches of his family merged to gain strength and glory through a strategic match between select parallel first cousins on the side of the father. Although unequaled for its richness, Ahimaaz's chronicle is not unique.[34] Numerous short genealogies from the Rhineland tell of sons who perpetuated the rabbinic traditions and scholarship of their forebears.

These genealogies have made possible a partial reconstruction of the five major Rhineland rabbinic families, including that of Kalonymides. One may even speak in a qualified sense of lineages[35] as well as observe the political and social strength that the members of these five families achieved through their status as communal leaders.[36] Many leading rabbinic figures in both Germany and France were also

[30] Grossman, *The Early Sages*, 260, claims that, in families listed as Mr. X, his son, and his brother, etc., the brother is Mr. X's. Because, however, these lists invariably go on to say "and their children" in one lump reference, including the children of the undisputed son of X, it seems to me that the brother is that of the son and not of X himself.

[31] There are references to siblings killing one another; still, the chronicles never indicate blood relationships between the nuclear families they describe; see, for instance, the case of Judah ben Isaac Machir cited in Salfeld, *Das Martyrologium*, 11.

[32] See *Sefer Ḥasidim* (Jehuda Wistinetzki, ed., *Das Buch der Frommen* [Frankfurt am Main, 1924]), nos. 949 and 950, which takes note of the dangers of fathers living with sons and the mirth of servants in a household where the parents of the wife were living in. Compare *Teshuvot Maharam ben Baruch* (Prague, 1608), no. 81, on friction between mothers and daughters-in-law in the same household; and see n. 112 below, for the view of Menahem ben Zerah.

[33] On the membership of the Rokeah in the Kalonymous family, see Ivan G. Marcus, *Piety and Society: The Jewish Pietists of Medieval Germany* (Leiden, 1981), 1; and on the genealogy of the Rokeah, see Joseph Dan, *The Esoteric Theology of Ashkenazi Hasidism* (Hebrew) (Jerusalem, 1968), 16 (English trans. in Marcus, 67).

[34] See Klar, *The Scroll of Ahimaaz*, 26–34, and *passim*.

[35] See these in Grossman, *The Early Sages*, 46, 91, 181, 261, and 302.

[36] Grossman, *The Early Sages*, 400–11; and Grossman, "Family Lineage and Its Place in Early Ashkenazic Jewish Society," *Studies in the History of Jewish Society in the Middle Ages and in the Modern Period*

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related. Yet true houses or clans, in the sense of those described by Jacques Heers, did not emerge.[37] It is their absence that accounts for the failure of the Crusade chronicles to specify lateral relationships in the first generation. In the chronicle of Ahimaaz, for example, siblings from whom the author was not directly descended are alluded to only briefly or are pointedly made to disappear.

The absence of clans, *domus*, or full-blown *consortia* may be seen again in the lack—Ahimaaz aside—of a strategy of family aggrandizement based on the marriage of cousins. Unlike Christians, Jews were free to marry cousins and nieces; in the Islamic East, first-cousin marriage among Jews was the norm.[38] In the Rhineland, however, such marriages were somewhat of an exception. This difference may be deduced from the universally accepted Communal Ordinance (*Taqqanah*) proposed by Jacob Tam, the most imposing Jewish authority of his day (d. 1171), on the return of the dowry should the bride die without issue during the first year of marriage. Fathers, the ordinance propounds, should not lose both their daughters and their wealth in one blow.[39] If most marriages had been between first cousins, the respective in-laws, who would also have been siblings, would normally have found ways of resolving issues of money among themselves without the need for legal sanctions. The Responsa (*consilia*) literature, too, legal questions and answers pertaining to actual litigations, supports this conclusion. Responsa may represent exceptions, but they are useful in terms of their specifics or when their decisions reflect precedent or common practices. Thus, in one case, an executor, who was (it should be stressed for its own importance) not a relative of the deceased, married his ward to *his* brother.[40] The brothers of the bride protested, not because she had been married to a non-relative but because they were concerned with the suitability of the match. Had marriage between cousins been the rule, it is doubtful that an executor, especially one who was not a relative, would have dared to violate it.[41]

Presented to Professor Jacob Katz on his Seventy-Fifth Birthday, E. Etkes and Y. Salmon, eds. (Jerusalem, 1980), 1–24.

[37] Jacques Heers, *Le Clan familial au moyen age* (Paris, 1974).

[38] Goitein, *The Family*, 27–33; for Spain, see Beatrice Leroy, *The Jews of Navarre* (Jerusalem, 1985), 126–30; and Maurice Kriegel, *Les Juifs à la fin du Moyen Age dans l'Europe méditerranéenne* (Paris, 1979), 131–33.

[39] The text is in Finkelstein, *Jewish Self-Government*, 139–40 and 169.

[40] Joel Mueller, *Teshuvot Ḥakhmei Ẓarfat ve-Lotair* (Vienna, 1881), no. 89, with English translation and commentary in I. A. Agus, *Urban Civilization in Pre-Crusade Europe*, 2 vols. (New York, 1965), 2: 568. Compare also Agus, *Urban Civilization*, 2: 427, 480, 521, and especially 427, a hypothetical discussion of the phrase: two men "related by marriage," in which the questioner failed to specify how they were related. The respondent suggests: brothers-in-law, married to two sisters, or married to two cousins. Such phraseology and explanations would make little sense were either of the men married to a cousin.

[41] See also *Sefer Ḥasidim*, no. 953; and compare Agus, *Urban Civilization*, 2: 431, discussing a newcomer to a town marrying locally; and Agus, *Urban Civilization*, 2: 427, in which even a wealthy communal official (*parnas*) marries outside.

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In regard again to Jacob Tam's Communal Ordinance (*Taqqanah*), a further observation is possible. If the parents of spouses had most often been siblings or kin, and the degree of potential strife between them still great enough to require a *taqqanah*, then to the extent that extended families existed, *consortia*, *domus*, or clans were not playing their role as mediators and so had little significance. Even their traditional primary function of preserving property and honor had become obsolete. Again with the caveat against generalizing from Responsa, it is possible to point to the bitter dispute that broke out between two brothers who had betrothed their children but then sued each other over breach of promise and the improper confiscation of the pledges that had been given when the betrothal was made.[42] From nearly all angles, therefore, it appears that the conjugal unit, the most prevalent family structure in Jewish society, was also the dominant one, taking precedence over all other family bonds.[43]

THE STRUCTURE OF THE ROKEAH'S FAMILY WAS THE NORM, but whether Christian families exhibited a parallel structure cannot be determined, as noted earlier.[44] Data of the kind supplied by the Mainz memorial list are not available for Christians. Georges Duby has noted that, "as soon as the historian leaves the thin upper crust of the society of this period, he enters impenetrable darkness."[45] Structures similar to those of the Jews have been identified only in Italy from the thirteenth century onward. These observations are too far removed chronologically and geographically to justify comparison.[46] A similar problem is encountered in discussing the Rokeah's esteem for the individual members of his family and the representativeness of the roles they played. Here, too, since he and his family emerge as paradigmatic, it is disappointing not to have contemporary Christian

[42] Mueller, *Teshuvot Ḥakhmei*, no. 27 (Agus, *Urban Civilization*, 2: 589).

[43] The ways in which nuclear families were affected through membership in rabbinical families remain to be studied; see Abraham Grossman, "From Father to Son: The Inheritance of Spiritual Leadership in the Jewish Communities of the Early Middle Ages" (Hebrew), *Zion*, 50 (1985): 189–220.

[44] Using admittedly meager data, L. K. Leyser, "The German Aristocracy from the Ninth to the Early Twelfth Century," *Past and Present*, 11 (1968): 25–53, especially 38, has identified an extended family structure and a system of infinitely partible inheritances quite unlike that of the Jews. Confirmation comes from Edith Ennen, *Frauen im Mittelalter* (Munich, 1985), 90–120, especially 100–02.

[45] Georges Duby, *The Knight, the Lady, and the Priest*, B. Bray, trans. (New York, 1983), 20; and compare Lutz K. Berkner, "Recent Research on the History of the Family in Western Europe," *Journal of Marriage and the Family*, 35 (1973): 395–405, especially 398–99, on the lack of demographic materials.

[46] See, for example, Diane Hughes, "Urban Growth and Family Structure in Medieval Genoa," *Past and Present*, 66 (1975): 3–28; and Hughes, "Domestic Ideals," 114–44; and compare Cinzio Violante, "Quelques caracteristiques des structures familiales en Lombardie, Emilie et Toscane aux XI^{e} et XII^{e} siècles," *Famille et parente*, G. Duby et J. Le Goff, eds. (Paris, 1977), 118–25; Paolo Cammarosano, "Les Structures familiales dans les villes de l'Italie communale (XII^{e}–XIV^{e} siècles)," *Famille et parente*, 190–91. Their claims about the importance of the nuclear family within the framework of the consortial are disputed by David Herlihy, "Family Solidarity in Medieval Italian History," *Economy, Society and Government in Medieval Italy*, D. Herlihy, R. S. Lopez, and V. Slessarev, eds. (Kent, Ohio, 1969), 177–78, and 181.

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parallels.[47] Comparisons of Jewish with Christian family behavior would no doubt throw light on such important questions as the mutuality of nuclear family patterns and the existence of affective bonding between family members. Comparisons might also highlight the role of the city and of urban political and social institutions in shaping family life.[48] Relationships of this kind may be seen in fourteenth and fifteenth-century Italy, where urban middle-class life is even said to have had a "bourgeois" quality.[49] But, once again, these Italian families are at too great a remove for fruitful comparison.

The European Jewish family had become quintessentially urban in the thousand years before the First Crusade, so much so, in fact, that it would be difficult to prove that the values of the Rhineland family were rural ones recently transferred to an urban setting.[50] Jewish occupations were characteristically urban: trade, artisanry, and lending—as were contemporary applications of the Jewish laws of inheritance. Although these laws had always divided the estate among all sons, awarding the first-born a share equal to twice that of his brothers, they now insisted on the daughter receiving 10 percent in the form of a dowry.[51] With the exception of houses and an occasional vineyard, moreover, these estates were composed exclusively of movable property. Even when Jews did own land, they do not seem to have worked it. Nor was the formation of consortial or joint families either desirable or a necessity in order to hold parcels together.[52] This type of division

[47] For early medieval Christian families, see Emily Coleman, "Medieval Marriage Characteristics, A Neglected Factor in the History of Medieval Serfdom," *Journal of Interdisciplinary History*, 2 (1971): 1–15; and especially the arguments of Carl Hammer, "Family and *Familia* in Early Medieval Bavaria," *Family Forms in Historic Europe*, R. Wall, J. Robin, and P. Laslett, eds. (Cambridge, 1983), 249. See, too, David Herlihy, "Land, Family and Women in Continental Europe, 701–1200," *Traditio*, 18 (1962): 94–98.

[48] See the proposals of Livi-Bacci, "Ebrei, aristocratici," 936. David Nicholas's *The Domestic Life of a Medieval City: Women, Children, and the Family in Fourteenth Century Ghent* (Lincoln, Neb., 1985) reached me too late for proper consideration in this article.

[49] See Alberto Tenenti, "Famille bourgeoise et ideologie au Bas Moyen Age," *Famille et parente*, 431–40. Like Herlihy in "Family Solidarity," 180–81, Tenenti cautioned that the "bourgeois family" is not to be confused with the twentieth-century family, which Herlihy called "autonomous," and Barbara Laslett, "The Family as a Public and Private Institution," *Journal of Marriage and the Family*, 35 (1973): 480–92, called the "private" family. For potential parallels (not, of course, identities) between medieval and later families, see Eli Zaretsky, "The Place of the Family in the Origins of the Welfare State," in *Rethinking the Family: Some Feminist Questions*, ed. by Barrie Thorne with Marilyn Yalom (New York, 1982), 188–224, on the early nineteenth-century family.

[50] See Heers, *Le Clan familial*; and the strictures of F. W. Kent, "A la recherche du clan perdu: Jacques Heers and 'Family Clans' in the Middle Ages," *Journal of Family History*, 2 (1977): 77–86, especially about the transfer of rural values. Some Jews lived in small towns, but these towns were linked organizationally, culturally, and judicially to the communities of larger urban centers.

[51] See Mueller, *Teshuvot Hakhmei*, no. 84 (Agus, *Urban Civilization*, 2: 581–82). See, too, the summaries on "Dowry," "Ketubah" (marriage contract), and especially "Primogeniture" in the *Encyclopaedia Judaica*. Compare Safrai, "Mivne ha-Mishpaḥa," 141–42, on similar inheritance patterns in the Talmudic period. On dowries and women's status in Christian society, see Diane Owen Hughes, "From Brideprice to Dowry in Mediterranean Europe," *The Marriage Bargain: Women and Dowries in European History* (New York, 1985), 24–28, 43–46.

[52] See Mueller, *Teshuvot Ḥakhmei*, no. 85 (Agus, *Urban Civilization*, 2: 573–74), for a vineyard owned—and worked—in pre-Crusade Troyes by a solitary widow. She may have been the only Jew in Troyes holding and working land. See Guido Kisch, *The Jews of Medieval Germany* (New York, 1949), 209–10, on Jewish ownership of real property in general.

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of labor, land, and wealth favored the growth of independent nuclear family units, as well as universal marriage.[53]

Perhaps also related to urbanization was this Jewish society's overall homogeneity. Distinctions of wealth, learning, power, and deference to members of rabbinical families—all of which existed—were distinctions of degree and not of kind or social class. Even members of the five select rabbinic families did not enjoy special legal or economic privilege.[54] Such a lack of hardened social strata may well help explain the absence of any correlation between Jewish family size and family standing in the community. It certainly removed artificial barriers preventing marriage between any two Jews.[55]

Contemporary legislation encouraged these patterns of behavior. New ordinances emphasized the rights of individuals and showed particular concern for the prerogatives of married women. In theory, the rabbinical authorities of the eleventh and twelfth centuries stressed, the husband was the senior partner.[56] But he was not to be a dominating patriarch whose sole duties were providing for the material and physical needs of his wife. Rather, he was obligated to ensure what the sources call "the tranquillity of the home."[57] Husbands were prohibited from departing on lengthy business trips in the heat of an argument, nor could they absent themselves from their homes for longer than six months at a time. Authorities also insisted on monogamy.[58] This insistence was paralleled by an open tolerance of divorce. Nevertheless, women could not be divorced against their will; they even enjoyed a measure of initiative. Despite the strict Talmudic ruling that the husband alone has the right to divorce, rabbinic leaders in the eleventh and twelfth centuries concurred that a woman could at least request a divorce—if only

[53] The point here is not that a lack of inheritance division prevented marriage, which has been disputed—see Herlihy, *Medieval Households*, 136–38, and Duby, *The Knight*, 276—but that partible inheritance encourages marriage. Jews were even conscious of this, as attested to by Simone Luzzatto in his *Discorso* (*Ma'amar 'al Yehudei Venezia*, A. Z. Ascoli, ed. [Jerusalem, 1951], 96), end of the "Seventh Consideration"; the Hebrew is a translation of the 1638 (Venice) Italian, for which no modern edition exists.

[54] See Grossman, *The Early Sages*, 411; and Grossman, "Family Lineage," 21, describing the breakdown of commercial monopolies and tax exemptions once enjoyed by scholars. If Agus's claim is correct, in *Urban Civilization*, 551, that in some towns the "eminent," that is, scholarly, members of the community were a majority, then the Jewish community was truly homogeneous. In Mainz, family heads were referred to as "R.," which Agus said means rabbi. Still, one would like more proof before accepting this.

[55] See, for example, Agus, *Urban Civilization*, 2:427. An extreme, if stylized, literary example is the match between the daughter of the wealthy Theophilo and a poor, righteous (and anonymous) doer of good deeds, in Klar, *The Scroll of Ahimaaz*, 25–26.

[56] The texts are in Finkelstein, *Jewish Self-Government*, 143, 145–46, 176, and 193.

[57] *Babylonian Talmud*, Tractate *Ketubot* (Vilna, 1895), 77a [henceforth, as per convention, *B.T.*, followed by the tractate's name; for a lucid introduction to rabbinic literature and commentaries, see the first chapter of Feldman, *Marital Relations*, cited in n. 7 above]; Menahem ben Zerah, *Zedah la-derekh* (Jerusalem, 1977), part 3, arg. 1, chap. 1; and see Samuel Morrell, "An Equal or a Ward: How Independent Is a Married Woman according to Rabbinic Law?" *Jewish Social Studies*, 44 (1982): 198, on the expression "No one can live with a serpent in the same basket."

[58] Finkelstein, *Jewish Self-Government*, 139, for the *Taqqanah* of Rabbi Gershom of Mayence (d. *circa* 1028), establishing the rule of monogamy; and see Falk, *Matrimonial Law*, 1–34.

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because she "loathed" her husband.[59] Correspondingly, the formal consent of women to marriage seems to have been obligatory.[60] Clandestine or forced marriages were almost always annulled.[61]

These legislative ideals, which were reiterated and applied as late as the sixteenth and seventeenth centuries in Italy, unquestionably enhanced women's prerogatives.[62] Their real purpose, however, may have been to bolster some particular marriage strategy, as has been said of the church canons governing marriage.[63] If so, the goal of that strategy could only have been to support the continued existence of an urban conjugal family. In the case of leviratic marriage, for instance, a clause guaranteeing the freeing of a childless widow from the obligation of marrying her brother-in-law became a fixture in marriage contracts. And after many trials, it was decided that the estate be split equally between the widow and the brother-in-law in order to discourage thoughts about marrying the widow for monetary advantage.[64] Jews had no incentives for developing a strategy favoring agnatic clans or the formation of a large patrimony in a single line. They were neither landed nobility nor farmers, and, on the rare occasions when they did possess political power, that power was personal, except perhaps in the case of Jewish political power in Spain. Jews did not rise in civil status through marriage, as had once been the case with slaves on ninth-century German estates; they were always legally and constitutionally Jews.[65]

The choice of a marriage partner, though important for reasons of prestige and wealth, was thus not as critical for Jews from the standpoint of enhancing or preserving a lineage or maintaining a landed patrimony as it was for Christians. Individual concerns could and did weigh no less than those of the group. The individual could wax so important, in fact, that in the fifteenth century, complaints arose about the relative ease with which certain German rabbis condoned the exchange of marital partners through divorce and quick remarriage for sexual motives alone.[66]

[59] Morrell, "An Equal or a Ward," 200.

[60] *B.T., Kiddushin*, 41a; see M. A. Friedman, "The Ethics of Medieval Jewish Marriage," *Religion in a Religious Age*, S. D. Goitein, ed. (Cambridge, Mass., 1984), 85–86; and *Ha-'Enziqlopedia ha-'Ivrit* (Hebrew), s.v. *Hafqa'at kiddushin* (annulling a betrothal), 7:404. Compare Christian theory and usage as reported by Goody, *Development of the Family*, 152 and 155. The church formally stressed this point more than the Jews; see Charles Duggan, "The Canon Law on the Formation of Marriage and Social Practice in the later Middle Ages," *Journal of Family History*, 8 (1983): 145–47; in practice, the rule was violated more often than not; as in Herlihy, *Medieval Households*, 207.

[61] See Finkelstein, *Jewish Self-Government*, 305, 317, 364–65; and Grossman, "Family Lineage," 19, for the Jewish attitude. Compare Beatrice Gottlieb, "The Meaning of Clandestine Marriage," *Family and Sexuality in French History*, R. Wheaton and T. Hareven, eds. (Philadelphia, Pa., 1980), 49–83.

[62] Archivio Storico Capitolino, Rome, Section III, Fascicle 11, vol. 3, ff. 94r and v; on similar decisions, see Kenneth R. Stow and Sandra Debenedetti Stow, "Donne ebree a Roma nell'età del Ghetto: Affetto, dipendenza, autonomia," *Rassegna mensile di Israel*, 51 (1986).

[63] See Goody, *Development of the Family*, 151–56; and Duby, *The Knight*, 123–85, and *passim*.

[64] Finkelstein, *Jewish Self-Government*, 145, 245–47, 254–55.

[65] See Gavin Langmuir, "*Tanquam servi*, The Change in Jewish Status in French Law around the Year 1200," *Les Juifs dans l'histoire de France*, M. Yardeni, ed. (Leiden, 1980), 24–54.

[66] Yisrael Y. Yuval, "An Appeal against the Proliferation of Divorce in Fifteenth-Century Germany" (Hebrew), *Zion*, 48 (1983): 177–216.

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This emphasis on privilege does not mean that Jewish legislation ignored the question of personal liability. The participation of Jewish women in business activities, for example, necessitated circumventing the rulings of the Talmud and allowing women to appear in court on their own behalf in claims relating to business losses. Some of these losses may have been the result of lending. Numerous records of loans made by Jewish women have survived from the thirteenth century.[67] These women lenders apparently had no counterparts among medieval Christian women, whose various economic activities do not appear to have included finance.[68] Reminiscent of Carolingian Christian women, whose small number tipped the scales of the marriage market in their favor,[69] this unique economic role of Jewish women strengthened their hand as marriage partners. And that hand was not weakened by the ability that these women lenders must have had in reading and writing. One Responsum states flatly that it is the obligation of the mother to oversee the arrangements made with the tutor of her three sons.[70]

That women capable of taking initiatives could insist on the observance of their prerogatives is not surprising. One woman had been married as an eleven-and-a-half-year-old minor and had been given less than the standard 10 percent dowry. On reaching maturity, she went into court to claim the difference—and apparently got it.[71] In another instance, a second wife successfully contested the attempt of her husband to disinherit her children in favor of those of his deceased first wife. He had claimed that the business activities of the second wife had made her sufficiently wealthy to provide for her offspring by herself.[72] In a third example, a thrice-divorced woman was partially successful in a suit to collect more of her marriage portion than her husband felt she was legally entitled to because of her presumed infertility.[73] On the death of a husband, or divorce, a woman automatically collected the often substantial value of her marriage contract, including the dowry and an additional jointure of indeterminate size promised by the husband.[74] Alternately, she could give a formal receipt (*moḥel*) and, in the

[67] W. C. Jordan, "Jews on Top: Women and the Availability of Consumption Loans in Northern France in the Mid-Thirteenth Century," *Journal of Jewish Studies*, 29 (1978): 39–56.

[68] On women's economic pursuits, see Eileen Power, *Medieval Women*, M. M. Postan, ed. (Cambridge, 1975), 53–76; and especially Ennen, *Frauen im Mittelalter*, 104 and 107, who points to the "subsidiary" character of women's work and admits "aber davon wissen wir vor 1250 wenig." Nicholas, *Domestic Life*, 84–87, reports some Christian women lenders in fourteenth-century Ghent, especially after 1360.

[69] See David Herlihy, "The Medieval Marriage Market," *Medieval and Renaissance Studies*, 6 (1976): 3–27.

[70] Mueller, *Teshuvot Ḥakhmei*, no. 99 (Agus, *Urban Civilization*, 2: 730); in fact, the mother's responsibility was financial, but the implication is that she can judge the quality of the teaching.

[71] Mueller, *Teshuvot Ḥakhmei*, no. 94 (Agus, *Urban Civilization*, 2: 581).

[72] Mueller, *Teshuvot Ḥakhmei*, no. 47B (Agus, *Urban Civilization*, 2: 668).

[73] Joel Mueller, *Teshuvot Geonei Mizraḥ u-Ma'arav* (Berlin, 1888), no. 166 (Agus, *Urban Civilization*, 2: 638). Husbands were required to make equitable settlements: see *The Responsa of Rashi*, Israel Elfenbein, ed. (New York, 1943), nos. 208 and 218 (Agus, *Urban Civilization*, 2: 682 and 695). Perhaps she was seeking to avoid pregnancy yet was able to remarry because of her beauty and desirability.

[74] See *Encyclopaedia Judaica*, s.v. "Dowry" and "Ketubah," for an outline of these laws. There are also indications that women owned property independently during the lifetimes of their husbands; see

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manner of an executor, acquire direct control over some share of the estate before any other claims were made.[75] She could even pass this property, or that from her dowry, on to her heirs, including those not related to her husband—a considerable modification of the rule of inheritance exclusively through the paternal line. On one notable occasion, a husband was granted the rare privilege of taking a second wife without divorcing the first, following ten years of marriage with no issue. At his death, the first wife inherited a house, which she then bequeathed to her nephews, the sons of *her* brother.[76]

A wife's inheritance rights were also broad enough to permit her to stand at the head of the household after her husband's death. Women comprised 27 (22.6 percent) of all family heads on the Mainz memorial list. This proportion is roughly the equivalent of the 21.4 percent of female heads of family in Pistoia in 1428. Certain Jewish women retained their positions at the head of the family even after their sons had reached maturity and married.[77] This concern for widows was matched by a desire to enable every woman to establish a family. One step in this direction was ensuring that dowries not become unthinkably small. Families gave priority to their composition, even if this meant reducing the inheritances of protesting sons or stipulating that a daughter be provided with a dowry before a widow collected the value of her marriage contract.[78]

Of course, congruence between law and reality was not always maintained. The rules regarding women—and occasionally men—could be compromised. A guardian affianced his female ward and had the courts ratify the act with no indication in the proceedings about her giving consent;[79] the courts decided that it was within the power of a father to force his son to marry a particular young

Morell, "An Equal or a Ward," 190–97; and Mueller, *Teshuvot Geonei*, no. 164 (Agus, *Urban Civilization*, 2:637).

[75] Mueller, *Teshuvot Geonei*, no. 190 (Agus, *Urban Civilization*, 2: 660) and no. 220 (Agus, *Urban Civilization*, 2: 640); and Mueller, *Teshuvot Ḥakhmei*, no. 194 (Agus, *Urban Civilization*, 2: 581). Compare Duby, *The Knight*, 95–106, on the loss of power by eleventh-century and twelfth-century women, including that of retrieving their dowries even if dismissed by their husbands. By contrast, women in Bohemia in the later fourteenth century appear to have been acquiring rights similar to those conferred on Jews by the act of "receipt"; John Klassen, "The Development of the Conjugal Bond in Late Medieval Bohemia," *Journal of Medieval History*, 13 (1987): 161–78.

[76] See Mueller, *Teshuvot Ḥakhmei*, no. 28 (Agus, *Urban Civilization*, 2: 663), for the general case; see no. 81 (Agus, *Urban Civilization*, 2: 663), for the example.

[77] See Mueller, *Teshuvot Geonei*, no. 198 (Agus, *Urban Civilization*, 2: 660) and *Teshuvot Ḥakhmei*, no. 85 (Agus, *Urban Civilization*, 2: 573), for specific examples of female heads of family. On Pistoia, see David Herlihy, *Medieval and Renaissance Pistoia: The Social History of an Italian Town* (New Haven, Conn., 1967), 249; see, too, Cammarosano, "Les Structures familiales," 193–94, on widows and property division in the case of minor children; and Duby, *The Knight*, 104.

[78] For reducing inheritances, see Mueller, *Teshuvot Ḥakhmei*, no. 85 and 94 (Agus, *Urban Civilization*, 2: 573 and 81); for sons who protested, Mueller, *Teshuvot Ḥakhmei*, no. 94 (Agus, *Urban Civilization*, 2: 581); and for provision for daughters, see Mueller, *Teshuvot Geonei*, no. 164 (Agus, *Urban Civilization*, 2: 637).

[79] Mueller, *Teshuvot Ḥakhmei*, no. 89 (Agus, *Urban Civilization*, 2: 568). Responsa families seem to have a greater number of children per family than martyr list families. Responsa, however, deal with exceptions, and in inheritance matters that often means a greater number of heirs. Mueller's texts, furthermore, are of cases heard before the equivalent of a high court of appeals.

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woman;[80] a man took the daughter of his sister for a wife;[81] and at least one eleven-and-a-half-year-old was living with her husband,[82] even though Jewish law does not consider a girl mature and marriageable until puberty, at age twelve and a half. Complications were also created by the high mortality of women in childbirth. The failure of children from a second marriage, especially of a father's children, to integrate and form a single unit with those of a first resulted in numerous litigations.[83] Family units properly included, or so many Jews in the Rhineland must have thought, only the biological parents and their common offspring, whose rights and concerns had to come first. This perception could only have been strengthened by the enunciated ideal that Jewish husbands, wives, and children should cultivate strong emotional bonds.

EMOTIONAL WELL-BEING WAS TO BEGIN IN INFANCY and continue to adult sexual fulfillment. The fourteenth-century Spanish guide to Halachic living, *Ẓedah la-derekh*, building on German-Rhineland precedents, advises mothers to sing to their babies in order to promote their happiness and contentment. Older children, too, were considered deserving of affection. Shefatiah in the chronicle of Ahimaaz "greatly cherished" his marriageable daughter.[84] Certain texts in the enigmatic thirteenth-century manual of the Rhineland Jewish Pietists fellowship, the *Sefer Ḥasidim*, suggest that the tender feelings of the Rokeah for his children were not unusual.[85] A sense of tenderness is also portrayed in the Crusade chronicles, which were composed in no small part to reenact for succeeding generations the severe emotional trauma suffered by Jews sacrificing their spouses and other relatives, but especially their children. Finally, there are passages in the writings of Rashi (d. *circa* 1104–14) and other commentators reflecting on love for a mother and its eventual transference to a wife, especially after the mother's death: "As long as a man's mother is alive, he is attached to [literally, bound up with] her; when she dies, he takes [physical and emotional] solace from his wife."[86] Nowhere, however, is the

[80] Mueller, *Teshuvot Ḥakhmei*, no. 13 (Agus, *Urban Civilization*, 2: 650).

[81] *The Responsa of R. Gershom*, Shelomo Eidelberg, ed. (New York, 1955), no. 37 (Agus, *Urban Civilization*, 2: 597).

[82] Mueller, *Teshuvot Ḥakhmei*, no. 94 (Agus, *Urban Civilization*, 2: 581); compare Elfenbein, *The Responsa of Rashi*, no. 198 (Agus, *Urban Civilization*, 2: 601), for a woman who was not betrothed until young adulthood.

[83] Mueller, *Teshuvot Ḥakhmei*, nos. 47B and 28; and *Teshuvot Geonei*, no. 164 (Agus, *Urban Civilization*, 2: 668, 663, 637). Instances of a distinct notation for children from a first husband appear in martyr lists from 1298, especially Nuremberg; Salfeld, *Das Martyrologium*, 32–37.

[84] Menahem ben Zerah, *Ẓedah la-derekh*, part 1, arg. 3, chap. 14. See also chap. 1, on the wife expressing "emotional affection" (*Ḥibbah*) and the husband an attitude of "honor and love" (*Yikhabdenah ve-ye'ehhavenah*). On Shefatiah, see Klar, *The Scroll of Ahimaaz*, 27. For Christian attitudes to children, see Mary McLaughlin, "Survivors and Surrogates," in Lloyd de Mause, *The History of Childhood* (New York, 1974), 101–28; and Herlihy, *Medieval Households*, 125–27.

[85] *Sefer Ḥasidim*, nos. 960–62; English trans. in Ivan Marcus, "Narrative Fantasies from Sefer Hasidim," *Fiction: Rabbinic Fantasy*, David Stern, ed., 7 (1983): 142–44.

[86] Rashi (whose comments are published in many standard editions of the Bible and Talmud) on Gen. 24: 67; see Rashi's additional comments in n. 111, below.

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desirability of emotional bonds uniting Jewish spouses more visible than in discussions of sexuality. These define not only the limits of acceptable sexual behavior but also the role of sexuality in generating proper affective relationships. In their writings, rabbis were addressing themselves, consciously or unconsciously, as if in a mirror, and setting standards for their own behavior as well as that of their followers. These standards were frank, permissive, and often openly sensual. As for the rest of Rhineland Jews, if they adhered to other standards, they could only have been ascetically more stringent and, with respect to the ideal, affectively wanting.

Jewish writers viewed sexuality in a remarkably positive light. When appropriately expressed within the framework of marriage, sexual activity required no apologies, transferences of meaning, or obligatory links with procreation.[87] This view recurs in texts from Talmudic times through the normative sixteenth-century code, the *Shulḥan Arukh*, and beyond. Decisions by Roman Jewish arbiters in 1540, for example, instruct feuding spouses, especially husbands, to love their wives as befits a husband and to unite with them in sexual union. Stress was placed on the achievement of sexual balance and mutuality, not the prevention of sin and the avoidance of the promptings of the devil. As summed up pointedly by Maimonides in his late twelfth-century code, the *Mishneh Torah*, and repeated by others: "A man should not be like a cock with his wife . . . but should make his cohabitation into an act of mutual joy."[88]

Especially pertinent is the discussion of the *mokh*, a form of cervical sponge used for contraception. All the authorities permitted its use in cases of danger to the woman; some, including the towering Jacob Tam, allowed its discretionary use as well. The thirteenth-century Italian commentator, Isaiah of Trani, and, apparently, the twelfth-century sage, Isaac of Dampierre, permitted its use if the intent of the partners was not to prevent conception but to enjoy sexual activity or preserve the beauty of the wife.[89] The universal need of both men and women for sexual release was unconditionally accepted.[90] Husbands were obligated to fulfill the *miẓvah* (religious obligation) of *'onah*, the timely—normally, weekly—gratification of the sexual needs of their wives. Sexual activity was also considered necessary to keep men's minds from wandering during study.[91]

[87] See Feldman, *Marital Relations*, 60–105, and especially, 63–64, 71–75.

[88] Maimonides, *Mishneh Torah, Hilkhot De'ot*, (Vilna, 1900) [henceforth, *M.T.*, followed by the category of laws Maimonides is discussing] 5: 4. Maimonides and other commentators are brought together in Jacob ben Asher's fourteenth-century code, the *Tur* ('Arba'ah turim) (Vilna, 1900), which leaves no doubt of both the fundamental unity and continuity, despite regional variations, of Jewish views on sexuality from Talmudic through at least early modern times.

[89] Commenting on *B.T., Yebamot*, 12b, in *Tosafot ha-RiD* (Jerusalem, 1931). Isaac of Dampierre, a nephew of Jacob Tam, is cited by Feldman, *Marital Relations*, 161–62.

[90] For a Christian view, see Joan Cadden, "It Takes All Kinds: Sexuality and Gender Differences in Hildegard of Bingen's 'Book of Compound Medicine,'" *Traditio*, 40 (1984): 167–69. Hildegard, who is certainly not paradigmatic, accepted the need of most (not all) men for sexual activity, yet she is more selective about the needs of women.

[91] On *'onah*, see Feldman, *Marital Relations*, 63–64, 71–75. On frequency, see Jacob ben Asher's *Tur*, Oraḥ Ḥayyim, section 240. On the link between study and sexuality and the naturalness of sexuality,

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Jews no doubt knew that their views on sexuality contrasted with the ambivalence of Christians. Egbert of Shonau, for example, cautioned that "sexual activity for purposes expressly not related to procreation is a major sin; that sin, however, is nullified by the salutary effects of marriage."[92] Jewish attitudes stand out even more sharply against Gratian's urgings of infrequent sexual concourse or his argument that "there is nothing filthier than excessive love of one's wife."[93] Especially perplexing for Jews were Christian attempts to renounce or sublimate sexuality.[94] One Jewish polemicist reacted with amazement, and no little derision, to the idea of lifelong celibacy, saying that priests and nuns must burn up in their unconsummated desires.[95] But perhaps Jews were aware of the differences often separating the ideals of Christian pastors from the practices and thought of the Christian laity.[96]

Christian pastors did speak of understanding between spouses,[97] but it is hard to imagine them explicitly linking sexual intimacies with the fostering of care and respect. The late twelfth-century authority, Abraham ben David of Posquières, in southern France, adopted precisely this position. In contrast to Christians who wrote that non-supine coitus was "unnatural" and even whorish and brutish, Abraham ben David saw the question of variant coital positions as an opportunity for affection.[98] He was not an exponent of sexual permissiveness—he frowned on oral sex; nor was he an advocate of equality among the spouses—he made it plain that "nothing was worse than a wife ruling her husband."[99] He insisted, nevertheless, that the true test for deciding on the propriety of specific coital

see Abraham ben David of Posquières, *Sefer Ba'alei ha-Nefesh*, J. Kafah, ed. (Jerusalem, 1964), 125–26. See also the aphorism of Rashi, *Lel Shabbat, Lel 'Oneg* ["Sabbath Eve, the night of sexual pleasure"], *B.T., Ketubot*, 62b.

[92] Cited in Jean Leclercq, *Le Mariage vu par les moines au XII^e siècle* (Paris, 1980), 21.

[93] Gratian cited in James Brundage, "Let Me Count the Ways," *Journal of Medieval History*, 8 (1984): 83 and 84.

[94] See Duby, *The Knight*, 179–83, on Hugh of St. Victor. On what amounts to a sublimation of sexuality into spirituality, see S. Antonio of Padua, *S. Antonii Patavini, Sermones Dominicales et Festivi*, Beniamino Costa, *et al.*, eds., 3 vols. (Padua, 1979), 2: 331; and Raymundus Martinus, *Pugio Fidei*, Johannes Carpzov, ed. (Leipzig, 1687), 354 and 758, on "Divine Seed."

[95] See David Berger, *The Jewish Christian Debate in the High Middle Ages* (Philadelphia, Pa., 1969), 69.

[96] For pastors, see John Thomas Noonan, *Contraception et mariage*, trad. M. Jossua (Paris, 1969), 254–55 (orig. published in English as *Contraception; A History of Its Treatment by the Catholic Theologians and Canonists* [Cambridge, Mass., 1965]); Duby, *The Knight*, 26–32, 62–63, 107–20, 139–56; Oronzo Giordano, *Religiosità popolare nell'alto Medioevo* (Bari, 1979), 113–25; and Brundage, "Let Me Count the Ways," 81–82; for laity, see Giordano, *Religiosità popolare*, 126–30; and Brundage, "Let Me Count the Ways," 87.

[97] See the zealous argument of Leclercq, *Le Mariage*, 104–05, and *passim*. See, too, John Thomas Noonan, "Marital Affection in the Canonists," *Studia Gratiana*, 12 (1967): 479–529; and David Herlihy, "The Making of the Medieval Family: Symmetry, Structure, and Sentiment," *Journal of Family History*, 8 (1983): 128.

[98] Brundage, "Let Me Count the Ways," 84; and see Duby, *The Knight*, 164, on women as a source of "unnatural sexuality."

[99] Abraham ben David, *Ba'alei ha-Nefesh*, 122–23. See, too, Maimonides, *M.T., Issurei Bi'ah*, 21: 9. Compare ben David's comment on *M.T., 'Ishut*, 21: 10, suggesting one may strike his wife; ben David objected: "I have never heard of such a thing." Some Jews, later on at least, had; Archivio Storico Capitolino, Rome, Sec. III, Fasc. 11, vol. 3, f. 42v.

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positions was mutual consent: "One may indulge in variant forms of coitus . . . but only when his wife consents; if he forces this upon her, it is sinful . . . Rather, he must act with her knowledge, endearing himself to her until she is willing."[100]

The context of these statements, a chapter entitled "The Gate of Holiness" in Abraham ben David's treatise on the laws of menstrual purity, is most revealing. Holiness is a recurrent theme in discussions of sexual behavior. It also appears in the *Letter of Holiness* (*'Iggeret ha-Qodesh*),[101] a kabbalistic tract with a prescriptive approach to the specific processes and procedures of coitus that became standard by the late Renaissance, alongside a greater regimentation in all areas of family life.[102] Like Abraham ben David, the author of the *'Iggeret* aimed at making the closest physical contact in life into a source of holiness and purity. Purity, he argued, is attained not through abstinence but through the punctiliously regulated performance of obligatory sexual intercourse. Sexuality, the very act of physical union, is a means toward achieving individual sanctity;[103] that sanctity, however, was indivisible from the cherishing of one's spouse.

This motif appears again in the works of the elitist and marginally unorthodox fellowship known as the Rhineland Jewish Pietists—of which the Rokeah was a major representative. Its manual, the *Sefer Ḥasidim* (*Book of the Pietists*), whose concern with sexuality surfaces repeatedly,[104] is exceptional only in its elaboration of a complex system of penances intended to combat uncontrolled sexual desire. The principal author, Judah the Pious, a member of the distinguished Kalonymus family of rabbis and gifted with a vivid imagination, warns of the man who "burns like a furnace" to have sexual relations, and he prescribes pushing the thumbs into the pew in front of him for the man who is aroused in the midst of his ritual devotions. Similarly, Judah provides a detailed scenario of a potential adulterer who "embraced, kissed, and fondled [his beloved's] entire body but refrained from having intercourse" despite their mutual desire.[105] More important than whether Judah believed such things occurred is his ability to imagine them. Neither he nor his contemporaries suffered from underdeveloped libidos. The Provençal commentator Isaac ben Yedaiah's description of a Jewish woman grasping at the

[100] Abraham ben David, *Ba'alei ha-Nefesh*, 122–23, adding, "He may do nothing with her that is against her will." See also Isaiah Tishby, *Mishnat ha-Zohar*, 2 vols. (Jerusalem, 1961), 2: 635; ben David was objecting to *B.T.*, *Nedarim*, 20b, which explicitly permits oral sex; and see the mild ambiguity of the *Tur*, *'Eben ha-'Ezer*, 25. See, too, Feldman, *Marital Relations*, 158–59.

[101] Pseudo-Moses ben Nahman, *'Iggeret ha-Qodesh, Kitvei ha-Ramban*, Ch. Chavel, ed. 2 vols. (Jerusalem, 1963), 2: 316–37.

[102] See Elazar Azqari, *Sefer Ḥaredim* (Venice, 1601), part 3, chap. 2, which is most available as cited in Isaiah Hurwitz, *Shnei Luhot ha-Brit* (Fürth, 1764), 102b.

[103] *'Iggeret ha-Qodesh*, 321, 324–26; and see Tishby, *Mishnat ha-Zohar*, 2: 644, "Through the act of union with his wife, he enables himself to cling [*dvekuth*, that is, achieves a state close to union but which is not union in the true sense] to the Shekhinah [the divine spirit]," and 2: 609 for Tishby's comments. The *Sefer Ba'alei ha-Nefesh* also speaks of "attaining holiness during intercourse," 112–13.

[104] Marcus, *Piety and Society*, 42–43, 46–47, and 79. See *Sefer Ḥasidim*, nos. 40–60.

[105] *Sefer Ḥasidim*, no. 53.

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genitals of her non-Jewish lover as she burned to have repeated intercourse and orgasm must be listed with the classics of sexual fantasy.[106]

Although uncontrolled sexuality provoked condemnation, Jewish writers gave every encouragement to a properly channeled sexual drive, to the point, in fact, of prescribing a proper diet to stimulate virility or to avoid vegetables that created bad breath and interfered with sexual pleasure.[107] Accordingly, works such as *Sefer Hasidim* focused on the idea of balance, seeing in a properly directed sexuality a necessary means toward achieving one of the fellowship's central goals, the observance of what its adherents called the "Will of the Creator."[108] Yet, here, *Sefer Ḥasidim* was reflecting a common Jewish outlook. What mattered was not frequency or the act of physical union itself but, rather, the intentions and attitudes expressed by spouses to each other during the act of intercourse. According to the mystical *Zohar*, a man must "speak sweetly to his wife, after he has received her consent, . . . so that their will be one, with no force involved." He must "attract her through love and unite [physically] with her; [as it is written in Gen. 2:24], man must leave his father and mother and cling to his wife." "With his kisses, there will be a union of his soul with hers."[109]

The source of anxiety for the Jews was physical union in the absence of appropriate feelings. As expressed succinctly by Abraham ben David, "Sexuality unaccompanied by the proper intentions becomes an evil rather than a good and a blessing, a sin rather than a source of reward."[110] Similarly, Rashi warned that one should not have relations with a wife who has become hateful. If a man hates his wife and thinks of another woman, the coitus is not "fulfilled" (literally, complete), and it is no better than fornication and prostitution. So, too, he admonished the brothers-in-law not to marry the childless widows of their brothers unless their intentions were strictly to fulfill the commandment of leviratic marriage; otherwise, they would be no better than adulterers. There is no possibility that Rashi's motives were ascetic ones. On at least one occasion, he spoke positively of women arousing their husbands, and husbands their wives, through the offering and caressing of the breasts and vagina.[111] Jewish views on sexuality

[106] The Hebrew text appears in Marc Saperstein, "The Earliest Commentary on Midrash Rabbah," *Studies in Medieval Jewish History and Literature*, I. Twersky, ed. (London, 1979), 294–97, with English translation and comments in Marc Saperstein, *Decoding the Rabbis* (Cambridge, Mass., 1980), 93–98. Ben-Yedaiah also rejects traditional ideas of *'onah* yet has an ideal view of marriage.

[107] See *Sefer Ḥasidim*, no. 1161; and *'Iggeret ha-Qodesh*, 329; also Rashi on *B.T.*, *Shabbat*, 140b.

[108] See Haim Soloveitchik, "Three Themes in *Sefer Ḥasidim*," *Association for Jewish Studies Review*, 1 (1976): 311–57, especially 312–25.

[109] Tishby, *Mishnat ha-Zohar*, 2: 635; compare Duby, *The Knight*, 183, for the completely spiritualized interpretation of this verse. See also ben Zerah, *Zedah la-derekh*, part 1, arg. 3, chap. 14, saying: "No one should come between a man and wife, even his father and his mother [citing Gen. 2:24]). When Gen. 2: 24 says, 'He shall leave,' . . . its meaning is not that the husband should desist from his honor and respect for them [his parents], but that such honor and respect should not keep him from clinging to his wife."

[110] *Ba'alei ha-Nefesh*, 116–22, especially 118.

[111] Rashi on *B.T.*, *Nedarim*, 20b, on the hated wife; *Shabbat*, 140b, on foreplay; and *Berakhot*, 13a, on the brother-in-law. Following the Talmud, Rashi indicated that children of a hated wife will be *physically*

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thus emphasized the importance of the individual as a partner in intense emotional transactions within the framework of marriage. Carried on in this sense, sexuality promoted what Jacob ben Asher in his great summarizing code of the fourteenth century, the *Tur*, described as the central purpose of marriage: "Whoever lives without a wife lives without well-being, blessing, home, Torah, a protective wall, and indeed without peace."[112]

The role of emotional involvement in marriage and, indeed, in the entire nexus of family relations, is given its utmost expression, however, in *Sefer Ḥasidim*. Judah the Pious proposed that a son consider whether the choice of a partner for love outweighs the obligation of respect for his father. Although few people may have ever had to confront this dilemma in reality, Judah's text reveals the depths of marital feeling that medieval Jews were capable of imagining. They were prepared to weigh the importance of these feelings against the observance of a fundamental religious and filial obligation.[113] Whether in daily life spouses were able freely to nurture their emotional attachments is another matter.

THE SMALL, URBAN, TWO-GENERATIONAL, CLOSE-KNIT FAMILY of the Rokeah, oriented to the needs and feelings of its individual members, thoroughly embodied the Jewish norm. But the family was only one of many institutions flourishing within the Jewish communities of the Rhineland. Beside it arose important schools of learning, which, in the eleventh and twelfth centuries, were the seats of communal power. These schools were primarily family institutions, however, headed and in large part populated by members of the five main Rhineland rabbinic families. These five families did not differ in their conjugal structure and nature from the rest of Jewish society. Nor did their members enjoy special legal or hereditary privileges.[114] It would, therefore, have been extraordinary had the family and public institutions of Jewish society in the Rhineland not been mutually supportive, even symbiotic. In fact, the legislative and pastoral guidelines

deformed, complementing Maimonides, *M.T.*, *De'ot*, 5: 4, that sexual behavior engaged in with "joy" ensures the birth of healthy and intelligent offspring. See also Tishby, *Mishnat ha-Zohar*, 2: 612 and 644. By contrast, Rashi's approximate contemporary, Hildegard of Bingen (Cadden, "It Takes All Kinds," 155), said that children conceived without love will be *spiritually* maimed.

[112] *Tur*, *'Eben ha-'Ezer*, 1: 1. Compare the nearly opposite, albeit considerably later, opinion on the sorrows of marriage: "*Ehestand ist Wehestand*," in Elfriede Moser-Rath, "Familienleben im Spiegel der Barockpredigt," *Predigt und soziale Wirklichkeit*, Werner Welzig, ed. (Amsterdam, 1981), 42–66, especially 53–54.

[113] *Sefer Ḥasidim*, no. 930, and especially 951; and, too, the text of Menahem ben Zerah, cited in n. 109. Compare Herlihy, *Medieval Households*, 132–33, on church perceptions of leaving parents for a spouse.

[114] See Grossman, "From Father to Son," 204–14; and Mordechai Breuer, *Rabbanut 'Ashkenaz be-Yemei ha-Beinayyim* [The Rabbinate in Ashkenaz in the Middle Ages] (Jerusalem, 1976), 40, 59, 63.

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produced in the Rhineland schools in support of the conjugal family demonstrate that such a symbiosis existed.[115]

The origins of this symbiosis may extend back chronologically and geographically to Italy in the ninth through eleventh centuries. The chronicle of Ahimaaz emphasizes the combination of personal learning, communal leadership, affective relationships, and conjugal structure distinguishing the families of his ancestors.[116] There was, moreover, an explicit link between the worlds of Italian and Rhenish Jews. The chain of learning and family traditions composed by the Rokeah commences with none other than Ahimaaz's foremost heroes.[117] What may have been transmitted north from Italy to the Rhineland, therefore, were traditions not only of intellect and learning but also of family and institutional life and leadership, with one set of traditions interacting with and fortifying the other. Ahimaaz even succeeded in translating those traditions into a vision of a properly ordered Jewish society. In the course of time, and especially in the small and well-educated communities of the Rhineland, where all Jews had at least minimal access to and control over communal institutions, these traditions became common property.[118]

These traditions were further strengthened by the necessarily limited nature of Jewish social and political institutions, lacking such basic powers as true jurisdiction, or even a constitutional definition of the rightful source of authority, and dependent on a common willingness to abide by the decisions of courts of Halachic arbitration.[119] A strong and self-contained conjugal family was needed to fill in where the institutions left off. In this sense, the Jewish family may perhaps have been paradigmatic. Should similar interactions between the family and public institutions be discovered in other medieval cultures, an important step may be taken toward identifying the conditions necessary for the growth of families which, like that of the Jews, might be called bourgeois or, more precisely, proto-bourgeois.

Some investigation of medieval family institutional interaction has already taken place. Thomas Glick has related the development of an urban culture to what he called a bourgeois family style in ninth and tenth-century Andalusia under the tutelage of the Muslim legal experts (*fuqaha*).[120] And Alberto Tenenti has claimed that bourgeois family development in Italy was given impetus by the growth of civic

[115] On possible relationships between social institutions and the family, see Levy and Coale, *Aspects of the Analysis of Family Structure*, 13–31; Natalie Davis's introduction in Duby, *The Knight*, vii–ix; and Berkner, "Recent Research," 395.

[116] See Klar, *The Scroll of Ahimaaz*, 27; Shefatiah, the leading scholar and political head in his day, has three children, a not-so-passive wife, lives in another structure, apart from that of his only brother, and cherishes his daughter. She marries the brother's son, but this was not a foregone conclusion. All of this is a literary description, but if Ahimaaz retouched reality, he had to be plausible.

[117] See Dan, *The Esoteric Theology of Ashkenazi Hasidism*, 16.

[118] Compare Robert Presthus, *Men at the Top: A Study in Community Power* (New York, 1964), 403, a sociological study of a twentieth-century, upper New York State town.

[119] See Finkelstein, *Jewish Self-Government*, 7–18.

[120] T. F. Glick, *Islamic and Christian Spain in the Early Middle Ages* (Princeton, N.J., 1979), 154–57, 197–99.

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humanism.[121] By contrast, countercurrents generating conflict and competition between families and institutions may have retarded affective conjugal family growth. The needs of the spiritual family of the church[122] at times clashed openly with those of the natural family, whatever its form. Pope Urban II at Clermont in 1095, for instance, summoned the assembled nobles to put aside their concerns for "children, parents and wives" and enroll in the crusading army in order to gain the eternal life promised in Matthew (10:37) to those who love God more than their mother and father.[123] Perhaps, therefore, it was not coincidental that the Italian affective conjugal family arose in a society in which the borders between sacred and profane and between ecclesiastical and lay jurisdictions were blurring,[124] and in which leaders like Coluccio Salutati sang the family's praise.[125] Future research may provide a satisfactory answer to this difficult question.

A satisfactory answer also remains to be found for a similar and final question regarding the family of the Jews. By the sixteenth and seventeenth centuries, especially in Central and Eastern Europe, Jewish society had developed a tendency toward social stratification and the establishment of quasi-hierarchical Jewish institutional and clerical structures.[126] As Jacob Katz summed it up, in theory, Jewish society was an open one; in fact, the possibilities for mobility were extremely limited.[127] The same may be said about the family. The openly affective family unit of the early Middle Ages appears to have been replaced by one that was patriarchally dominated and highly prescriptive in matters of emotion and sexuality.[128] A conscious strategy of matchmaking that paired learning and wealth began to widen and perpetuate social gaps.[129] Rhineland Jews had been conscious of these possibilities, but they had never pursued them with consistency or dedication.[130] Jewish family structure had apparently undergone modification in

[121] Tenenti, "Famille bourgeoise et idéologie," in *Famille et parente, passim*. See also Herlihy, *Medieval Households*, 116–18.

[122] The term, indicating the clergy, appears in Herlihy, *Medieval Households*, 122–23; and Robert Brentano, *Rome before Avignon* (New York, 1974), 171–210, also with a sense of opposition to the "natural family." Here, "spiritual family" is intended as co-terminus with the body of the faithful under clerical leadership. On alternate families, based on other than lineal blood relationships, see Lloyd A. Fallers, "The Range of Variations in Actual Family Size," in Levy and Coale, *Aspects of the Analysis of Family Structure*, 70–82.

[123] See *Roberti Monachi Historia Hierosolimitana*, *Recueil des Historiens des Croisades*, *Historiens Occidentaux*, 5 vols. (Paris, 1844–95), 3: 728.

[124] See Marvin Becker, *Florence in Transition*, 2 vols. (Baltimore, Md., 1968), 1: 40–41, 192, and 226–27; Herlihy, *Medieval and Renaissance Pistoia*, 248–57, especially 247, on "Civic Christianity."

[125] Herlihy, *Medieval Households*, 116–17.

[126] See Haim Hillel Ben Sasson, *Hagut ve-Hanhagah* (Jerusalem, 1959), 133–254, on these structures; and Katz, *Tradition and Crisis*, 199–209, for their theoretical bases. For initial confirmation of the discussions of Ben-Sasson and Katz, see Toch, "The Jewish Community of Nuremberg," 60–72, especially 65–66, where, in distinction to what was seen in the Rhineland, family size is directly related to factors of wealth and social standing.

[127] Katz, *Tradition and Crisis*, 209.

[128] On such families in Christian society, see Lawrence Stone, *The Family, Sex and Marriage in England, 1500–1800* (London, 1977); for Italy, see Tenenti, "Famille bourgeois," 438–40.

[129] Katz, *Tradition and Crisis*, 205–07; but see the strictures of H. H. Ben Sasson, "Musagim u-misi'ut be-historiah yehudit be-shalhei yemei ha-beinaiyim," *Tarbiz*, 29 (1960): 297–312.

[130] See Mueller, *Teshuvot Hakhmei*, no. 89 (Agus, *Urban Civilization*, 2: 568).

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response to changes that had generated discontinuities between the family ideal as it had once existed in the medieval Rhineland and the ideals governing the institutions and leadership of eastern European Jewish society in the early modern period.[131] But, if so, the Jewish family that has commonly been called "traditional"[132] should perhaps be more properly considered an early modern or even regional development, and the medieval family portrayed in the dirge of the Rokeah should be recognized as something quite distinct.

[131] Additional factors to consider are forced migration (expulsions), the deurbanization of Polish Jews who lived on noble estates and in small towns, and population growth. See Andrejs Plakans and Joel M. Halpern, "An Historical Perspective on Eighteenth-Century Jewish Family Households in Eastern Europe: A Preliminary Case Study," *Modern Jewish Fertility*, Paul Ritterband, ed. (Leiden, 1981), 18–32, showing over 50 percent of families with three or more children and 22 percent multigenerational—10 percent more than Mainz, a figure perhaps related to more dominant fathers.

[132] See the use of this term throughout Katz's work, and again explicitly by Etkes, "Family and Study of Torah," 87, and *passim*. Most recently on the early modern and modern Jewish family, see *The Jewish Family, Myth and Reality*, Paula Hyman, ed. (New York, 1985).

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The Eccentricity of Wheels, or Eurasian Transportation in Historical Perspective

WILLIAM H. MCNEILL

EVEN IN THIS AGE OF AIR TRAVEL, WHEN WE THINK OF TRANSPORTATION, we think first of wheeled vehicles—trucks, trains, or cars—and then of ships that sail across open water. This vision of how goods and people travel is accurate enough when applied to most of the European past, and Europeans were able to impose their style of transportation, and much else, on the rest of the world in the course of the past century and a half. But a broader historical perspective makes the familiar European definition of transport misleading because it omits animal portage—the caravan—and overlooks the sporadic importance of canals. This essay will attempt to redress the balance by putting the peculiarities of European transport systems into their appropriate world historical framework.

Edward Fox, in his *History in Geographic Perspective: The Other France*, emphasized the difference between movement by water and movement by land.[1] He pointed out that, before the railroad changed traditional patterns, water transport was both much cheaper and much more capacious than overland haulage. As a result, according to Fox, two diverse societies arose within France: one outward-looking, commercial, and maritime; the other rural, politically subject, and intensely local in economic matters. Links between the two were mainly political and quite slender: hence his subtitle, "*The Other France*." Fox may have exaggerated the separateness of rural from urban France, but his emphasis on the defects of European overland transport in the pre-industrial age is surely well taken. Before the second half of the eighteenth century, when the French pioneered systematic road building, European roads were little more than a succession of mud holes. During much of the year, the moist climate made roads all but impassable for wheeled vehicles, especially wagons carrying heavy goods.

In contrast, abundant precipitation combined with the almost flat topography of the north European plain to provide Europeans with a natural substitute for roads—the river system, which, with remarkably little artifical improvement, allowed easy movement far inland, thanks to comparatively placid currents and a fairly even flow of water throughout the year. Indeed, Europeans could put up with backward and inefficient overland transport before the eighteenth century only because the natural system of internal waterways was so good. Why bother

[1] Edward Fox, *History in Geographic Perspective: The Other France* (New York, 1971).

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with road building when a few boats could exploit nature and carry people and goods through so much of the landscape?

Coastal voyaging linked one river basin with another, but that required transshipment. River vessels were not seaworthy; indeed, northwestern Europe abutted on one of the stormiest and most tide-beset seascapes of the earth, so that navigators who had mastered the art of sailing on the North Atlantic and its adjacent waters had little to fear elsewhere. The remarkable rapidity of European expansion around the globe after 1492 attested this fundamental fact. But Europe's extraordinary expansion after that date was matched by the slowness with which an effective network of seagoing ships, uniting separate river systems, had arisen in earlier centuries. Sea travel had long remained seasonal, confined to the summer months. Only toward the close of the thirteenth century did improvements in shipbuilding allow seafaring to become a year-round occupation for full-time specialists.

The technicalities and historic horizon points of European navigation and shipbuilding are well known. More than a generation ago, scholars asked why sixteenth-century Europeans had been able to conquer the high seas so suddenly, and underwater archaeology has recently added important new data, especially for ancient times.[2] I will not try to summarize the story here but wish to point out that the peculiar pattern of European transport before the railroad age gave unusual importance to port cities located at or near the mouths of major river systems. Since transshipment was necessary, re-sorting goods at quays and warehouses added little to the cost of unloading and reloading. It was logical to concentrate the manufacture and processing of goods in transit at places where they had to be sorted and re-sorted on the way to market. Fox's commercial and urban society resulted, which was located primarily or perhaps exclusively in port cities.

Other transport systems in which transfer from river boats to seagoing vessels was either unnecessary or unimportant could not sustain port cities to match the European model. Much of the uniqueness of medieval and modern European history rests on this peculiarity of geography. The transport system of the northwesternmost part of the European peninsula focused resources and economic functions in port cities—most of which, because of their peripheral locations, were not capitals of major states. The quasi-autonomy that European merchants and capitalists long enjoyed reflected their geographical separation from the principal seats of military and political power, a separation dictated by Europe's unique pattern of transport. Conversely, the imperfect control political authorities exercised over long-distance trade, and the wider range for market behavior this persistent weakness permitted, allowed European technology and commerce to flourish more luxuriantly than was possible in more closely regulated societies whose transport patterns did not isolate and insulate wealthy port cities from political-military management.

[2] Lionel Casson, *Ships and Seamanship in the Ancient World* (Princeton, N.J., 1971); Romola Anderson and Roger C. Anderson, *The Sailing Ship: Six Thousand Years of History* (London, 1926); John Guilmartin, *Gunpowder and Galleys* (Cambridge, 1975).

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Thus, for northwestern Europe, Fox's dichotomy between peripheral trading cities and a politically articulated rural hinterland was profoundly important. But it was unique to Europe—and, indeed, to only a part of that continent. Most of Spain, together with Italy south of the Apennines, lacked navigable river systems and port cities to match. Southern Italy and the Balkans had plenty of harbors but no river-mouth ports like those of northwestern Europe, with the partial exception of Thessaloniki, whose river valley hinterlands were, however, more often traversed by caravans than by river boats. One might argue that the city of Constantinople constituted the only real Balkan analog to western European port cities. Its river system embraced all the tributaries to the Black Sea, and, for practical purposes, the coastlands of the Aegean as well—an incomparable port but also, and for that reason, an imperial capital from 330 A.D. until 1920. Autonomy for Constantinople's commercial community was inconceivable under the circumstances. When the blighting effect of political management became too obvious, first the Byzantines and then the Turks permitted intrusive West Europeans to take over the export trade of their respective empires, deliberately conceding to foreigners commercial privileges denied to their own subjects![3]

Further north in Eastern Europe, the Volga and its tributaries flowed into a closed internal sea—the Caspian. This geography imposed a different transport pattern from that of northwestern Europe, whose rivers emptied into open water. Trans-Caspian trade connected to the caravan world, while portages from the Volga upwaters to rivers flowing into the Baltic and Black seas were short and fairly easy in winter, when freezing temperatures made the ground firm and snow smoothed a path for sleighs. This pattern of transport meant that Astrakhan, the Volga port city, was far less important than it would have been if the Caspian had been connected with the high seas. In addition, a large number of locations along the upper reaches of tributaries to the Volga could serve as places from which sleighs might cross over into the drainage areas of the Baltic and Black seas. Traffic on the Volga river system, therefore, did not sustain a concentration of commercial activity and manufacturing in a single port city. Instead, the supremacy of a political center, Moscow, became easier to impose and sustain, using the river-and-portage transport system of the Russian lands as arteries for both military enterprise and tax collection.

But Muscovite centralization also availed itself of overland transport, which played a far greater role in Eastern Europe than it did in the mud-splattered, rain-drenched landscapes of the European far west. This, in turn, reflected Russia's propinquity to the Eurasian grasslands and to the caravan transport system that prevailed throughout the steppe and desert lands of central Asia and North Africa. Let me therefore try to sketch that transport system, whose extent and character are far less familiar to historians and less carefully studied than they deserve.

Caravan transport is very old. Thousands of cuneiform tablets excavated at Kultepe in modern Turkey, dating from the twentieth century B.C., reveal a

[3] W. H. McNeill, *Venice: The Hinge of Europe, 1081–1797* (Chicago, 1976).

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well-organized system of trading caravans that connected the city of Ashur on the Upper Tigris with central Asia Minor. Strings of donkeys carried metals and textiles back and forth across hundreds of miles, and commercial practices for dealing with local rulers, warehousing goods, extending credit and sharing risks were surprisingly sophisticated. Merchants bought and sold at a distance. This necessity explains the existence of the tablets, since instructions had to be sent to agents on the spot, and formal records of agreements and transactions were needed to safeguard the interests of absent partners.[4]

About a century after this caravan system had reached its apogee, wheeled transport achieved extraordinary prestige and military importance through the invention of light and fast chariots whose wheels, complete with hub, spokes, and tire, turned on a fixed axle in the fashion familiar today. Such vehicles appeared in Mesopotamia about 1800 B.C. and for half a millennium thereafter dominated the battlefields of Eurasia. Conquerors, equipped with horses and chariots, spread their dominion across the ancient Near East within a couple of centuries. Rulers far and wide sought to command the services of suitably skilled wheelwrights and struggled to possess horses needed to pull chariots. As a result, chariots spread to both India and China by about 1500 B.C., and isolated examples of the wheelwrights' new art appear in Swedish burial mounds about a century later.[5] Tsetse-infested Africa and the inhospitable Eurasian taiga presented ancient charioteers with impassable barriers to their otherwise victorious expansion within the Old World. And, of course, they never reached the New. But within very broad regions of Eurasia, charioteers spread the idea of wheeled transport and the skills necessary for the manufacture and use of carts and wagons.

Western Europe's historic attachment to wheeled transport was a heritage from that chariot diffusion; northern India, central Asia and northwest China likewise preserved wheeled transport throughout subsequent centuries, using a variety of animals to pull two-wheeled carts whose design derived directly from chariots. In India and central Asia, however, wheels had to compete with caravans, because, in the center of the ecumene, technological advances in the management of camels, centering in Arabia, made wheels obsolete after about 300 A.D. As this transformation occurred, what I have called "the caravan world" came into its own. It continued to flourish with considerable vigor until the eighteenth century and then decayed at an accelerating rate until, in the second half of the twentieth, airplanes and desert-worthy motorized vehicles combined to displace camels even from their original homeland in Arabia.[6]

The early history of camel domestication is obscure, but, according to Richard Bulliet, camels were first used as milking animals by inhabitants of southernmost Arabia, perhaps as early as 3000 B.C.[7] Millennia later, camels became important beasts of burden when what Bulliet calls the "north Arabian saddle" was invented

[4] M. T. Larsen, *Old Assyrian Caravan Procedures* (Istanbul, 1967).

[5] William H. McNeill, *The Rise of the West: A History of the Human Community* (Chicago, 1963), 104–09.

[6] See William R. Polk and William J. Mares, *Passing Brave* (New York, 1973), for a vivid, nostalgic record of a venture across Arabia by camel caravan in 1971.

[7] Richard W. Bulliet, *The Camel and the Wheel* (Cambridge, Mass., 1975).

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sometime between 500 B.C. and 200 A.D. This device, whose design was probably influenced by cavalry saddles, rested on the animal's rib cage, thereby assuring riders of a firm seat atop the flabby hump and, incidentally, distributing the load more or less equally between fore and hind legs. The result was to make camel riders much more formidable in war, while also increasing the burden a single camel could carry to somewhere between 300 and 500 pounds.

Strings of camels, carrying such loads, cost less to maintain than wheeled vehicles—perhaps 20 percent less, if figures from Diocletian's Edict on Prices (301 A.D.) are well founded. Camels, horses, mules, and even donkeys could travel cross-country at a far brisker pace than any cart, and did not need specially constructed roads or bridges, since they could traverse nearly any terrain and ford most streams without difficulty. Caravanserais, places to deposit goods safely while animals and men were resting and eating, were the only important infrastructure that caravans needed, and that type of rest place was just as important for wheeled transport.

Once camel breeding spread widely enough throughout Arabia to assure an abundant supply of the animals, the cost advantages allowed camel caravans to displace carts and wagons from the lands of the Fertile Crescent. This happened between about 300 and 500 A.D. Farther away from the Arabian breeding center, camels had to be imported and so remained more expensive. As a result, camel transport dominated but did not entirely displace oxcarts and other wheeled vehicles in Asia Minor and northern India until the nineteenth century, when European patterns of mechanical transport intruded. To the east, two-humped or Bactrian camels, whose center of domestication appears to have been in Turkestan, played a role analogous to that of the Arabian camels in the Middle East, with the difference that they never entirely displaced wheeled vehicles from central Asia, much less from China and other lands of the Far East. Bactrian camels, when crossbred with Arabian camels, produced transport animals superior to either parent stock, much as mules are, for many purposes, superior to either horses or donkeys. Crossbred camels, unlike mules, remain fertile, so an indefinite series of crossbred strains eventually arose, each with particular advantages in different terrains and for different purposes.[8]

Long-distance transport was what camels excelled in, plodding day after day cross-country, almost regardless of the quality of grazing available to them along the way. A single attendant could manage about six animals. Commonly, strings of twelve camels, tied head to tail, were guided by one man in front and guarded by a second in the rear. Large caravans combined any number of such independent strings of camels, but the increased difficulty of finding water and forage along the way meant that big caravans made sense only when safety along the route was the major consideration. In such cases, a caravan leader assumed plenary powers over the entire expedition. But these large-scale, semi-military enterprises, however important for the pilgrimage to Mecca, were fundamentally uneconomic and so

[8] For authoritative data on camel biology and behavior, see Hilde Gauthier-Pilters and Anne Innes Dagg, *The Camel: Its Evolution, Ecology, Behavior and Relationship to Man* (Chicago, 1981).

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mattered less in ordinary life than the single string of six to twelve camels accompanied by one or two camel drivers.

Camels walk at a fairly brisk pace. In nearly any terrain, they travel approximately twenty miles in about six hours without difficulty. Longer distances in a day were possible but exceptional, since wherever wild herbage grew along the way, part of each day had to be devoted to foraging, so that the animals could refuel for the next day's march. This meant that in most Middle Eastern landscapes, where semi-arid wasteland abounded, caravans captured free energy just as sailing ships did. Thus caravans could often compete with ships on fairly even terms. They did so for about a thousand years, from the time when the arts of camel management became firmly established in the Middle East during the early Christian centuries until improvements in ship design and techniques of navigation changed the terms of competition after 1300 A.D.

To be sure, not every landscape offered free fuel to foraging camels, and any heavily traveled route soon became denuded. But Islamic societies invented ingenious ways of overcoming these obstacles. In truly desert lands, camels could, depending on temperatures, endure four to nine days without water and could go far longer than that without food by drawing on the fat stored in their humps. To regain their vigor and usefulness, the animals had to restore their fat reserve by resting for a month or longer in a place where fodder abounded. But the capacity of camels to maintain their muscular strength without taking in any food for a period of weeks meant that regions otherwise impenetrable, such as the Sahara and the "empty quarter" of Arabia itself, could be crossed by experienced camel drivers who knew where the necessary water could usually be found.[9]

The economic and cultural geography of Africa and, to a lesser degree, of central Asia, was profoundly altered when camel caravans made the deserts penetrable. The realm of Islam became capable of spanning barren deserts in the same way that European civilization later became capable of spanning vast oceans. The resulting shock to previously isolated populations in sub-Saharan Africa and in the remoter oases of Asia was nearly as great as the shock European sailors and settlers later administered to indigenous American and Australian populations.

In more densely inhabited regions of the Middle East, where cultivated fields covered the ground, caravans confronted not barrenness but an opposite problem in availing themselves of the free fuel. Camel drivers who let their camels loose to graze on local gardens and grain fields could expect swift retaliation. In a populated landscape, a foraging camel constituted wealth, free for the taking, if its master were not close at hand; hence, a camel driver had to worry about safeguarding his animals. Muslim law was summary and rigorous against theft, but that alone scarcely sufficed to keep transport animals safe if they were let loose to graze in cultivated landscapes.

Muslim law found a remarkable solution to the problem of feeding and safeguarding caravan animals in densely occupied regions and along heavily

[9] On Saharan caravans, compare James Willard, *Samarcand and Beyond: A History of Desert Caravans* (London, 1977), 31 and *passim*.

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traveled routes by making it an act of religious merit to endow caravanserais with income-producing properties. Such subsidies were exempt from taxation, thereby allowing the diversion of equivalents of rents and taxes to the supply of food and forage to travelers. Where such endowments existed, itinerant merchants and camel drivers could count on finding food, shelter, and fodder awaiting them free of charge for a stopover of not more than three days. The number of such subsidized caravanserais varied from time to time, but, whenever powerful Islamic empires guaranteed the legal order, the effect along heavily traveled routes was to reproduce, or very nearly reproduce, the costless fueling of transport that characterized the semi-arid, thinly occupied regions of the Muslim world, where natural herbage was available to caravans cost-free and without human intervention. Anything resembling quantitative information from the early centuries of Islamic history seems lacking. In the sixteenth century, however, when the Ottoman and Safavid empires divided the heartlands of Islam between them, and devoted much effort to the suppression of brigandage and the encouragement of trade, as many as 1,100 endowments for the benefit of travelers existed in Asia Minor alone.[10] Most of these endowments only supported humble establishments without any accommodation for animals. And we may assume that some travelers found no room at the inn, even where endowed caravanserais and hospices were best maintained and most numerous.

All the same, subsidized accommodation drastically reduced costs for caravan transport on major trade routes. The existence of conveniently located, well-endowed caravanserais explains otherwise incredible figures for transportation costs that have been derived from a few surviving account books of European merchants dating to the sixteenth and seventeenth centuries. Thus Niels Steensgaard, one of the few scholars who has studied the economics of caravan transport, concluded from an English merchant's records that a journey of seventy-nine marching days between Kashan in northern Persia and Bursa in Turkey, made in 1581–1582, cost him not more than 3 percent of the sale price of the silk he transported. Customs dues and protection costs totaled about twice that amount. The distance traversed was about 1,500 miles as the crow flies and must have been a good deal more on the ground. Less exact accounts of caravan transport costs appear comparable to this extraordinarily low figure. Steensgaard called his calculations "astonishing" and remarks: "If there were 600 piastres to be earnt [*sic*] on this route, it was at any rate not the camel that got the money, not even the tax-gatherers."[11]

In the absence of both free grazing and subsidized caravanserais, camel drivers had to buy fodder for their animals at whatever price local landowners or other authorities saw fit to charge. Travelers were at a serious disadvantage in such transactions, since their needs were urgent and their armed strength could seldom match that of local chieftains and rulers. Protection costs and the prices local

[10] Halil Inalcik, *The Ottoman Empire: The Classical Age*, Norman Itzkowitz and Colin Imber, trans. (London, 1973), 147–50.

[11] Niels Steensgaard, *The Asian Trade Revolution of the Seventeenth Century: The East India Companies and the Decline of the Caravan Trade*, (Chicago, 1973), 31–33.

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suppliers charged for food and fodder were, in fact, inextricably intertwined whenever local populations found it safe to exploit to the full their advantage over wayfaring strangers. When merchants had to pay such costs, therefore, caravan transport could not compete with shipping, since, once at sea, ships were immune from local dues and charges. Documents that show very high overland transport costs, even for quite short distances, presumably reflect a situation in which no overarching imperial authority was able to safeguard merchant travelers from heavy local exactions.[12]

The economics of caravan transport also afforded a middle ground between the costless fuel of open pasturage or subsidized caravanserais, on the one hand, and the ruthless local exploitation of needy travelers on the other. One such pattern is illustrated by Jean-Baptiste Tavernier's vivid account of how caravans were managed in Persia in the seventeenth century:

> When the caravan arrives at the place where it must camp, all the camels that belong to the same owner assemble in a circle, and kneel on all fours in such a fashion that when the rope that holds their burdens is untied, the packages sink gently to the ground on either side of the animal . . . After the camels are unloaded, they are allowed to wander into the open country to find something on which to browse, and a half hour before sunset they return of their own accord; or if by chance one wanders, it is easily recalled by a special cry. When they have returned and arranged themselves in a circle, each camel gets two balls of kneaded barley flour about the size of two fists.[13]

Such an arrangement allowed the caravan to pick up free energy from the landscape while supplementing what the camels could find for themselves along the way with barley meal carried as part of their load. By carrying a condensed fodder supplement with them, purchased at the most advantageous time and place within reach, camel drivers could thus hope to keep feed costs under control and reduce their dependence on the whims of local suppliers.

Protection costs, being infinitely variable, were more critical for the management of caravans than the predictable costs of providing fodder. The principal caravan routes of the Middle East and central Asia cut across desert or semi-desert country, shortening the distance between main centers of population and using resources otherwise exploited only by nomads. Payoffs to local tribesmen were a cost of using these routes, but, after the rise of Islam, and perhaps even before Muhammad's revelation gave a new moral basis to Arab social relations, merchants and nomads had little difficulty in concluding mutually advantageous arrangements in the Middle Eastern heartland. The Bedouin were camel raisers, after all, and caravan drivers must often have grown up in Bedouin tents before going off to seek their fortunes by serving rich merchants and haunting the city bazaars. Intimate

[12] See S. D. Goitein, *A Mediterranean Society*, vol. 1, *Economic Foundations* (Berkeley, Calif., 1967), 278; and Suraiya Faroqhi, "Camels, Wagons and the Ottoman State in the Sixteenth and Seventeenth Centuries," *International Journal of Middle East Studies*, 14 (1982): 531.

[13] Translated from Jean-Baptiste Tavernier, *Voyages en Perse* (Paris, 1970), 35. The original edition was printed in 1679, dictated in Tavernier's old age after more than thirty years of travel in Persia, India, and Turkey. It is interesting to compare Tavernier's account with the camel ration prescribed by the Turkish army during World War I, which was 4 kilos of barley and 9.4 kilos of straw per day, according to Suraiya Faroqhi, "Camels, Wagons and the Ottoman State," 529.

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symbiosis between urban and nomadic elements in Middle Eastern society was, in fact, an essential substructure for smooth operation of the caravan transport network.

The class that suffered was the peasantry, and it is not entirely surprising that the area of cultivation in the Middle East seems to have diminished during the caravan era in proportion to the increase in the social and military importance of nomadic tribesmen and urban merchants. Fluctuations in climate may have played a part in altering the boundaries between cultivated and uncultivated lands, as did population growth and catastrophe. But the social alliance of urban traders with nomads, exemplified in the career of Muhammad and stabilized subsequently by the entire Islamic political and legal system was both inimical to plowmen and favorable to the propagation and maintenance of the caravan network. It is no exaggeration to say that after about 700 A.D. the caravan world and the world of Islam became almost co-terminous.

From the beginning, successful conduct of long-distance trade by caravan required protection from rulers and raiders along the way. The Kultepe tablets describe in detail how merchants paid off local rulers in Asia Minor, used palace warehouses to store their goods and, in general, shared their profits with the possessors of political power. Sailors confronted comparable obstacles every time they came ashore or encountered a pirate crew at sea. But protection costs for ships at sea were usually less troublesome than for caravans, if only because a ship traversed uninhabited expanses, whereas a caravan was seldom far from populated places, and caravans concentrated wealth in a way that tempted innumerable plunderers.

Absolute security was unattainable. Each caravan had to balance the cost of paying for a formidable military escort against the cost of buying safe passage through the territories of local rulers or robbers. If greedy armed men set protection costs too high, trade languished or came to a halt. If protection payments were insufficient to dissuade successful attacks, disaster also ensued. A rough market in protection costs thus established itself throughout the caravan world whenever caravans crossed from one political jurisdiction to another. Little by little, more and more people came to understand that extracting a little from passing caravans might be worth more in the long run than risking all on a single, sudden attack. As that understanding hardened into custom and law, the security and predictability of caravan trade improved.

Nomadic herdsmen, owing to their mobility, were the most formidable enemies merchants and settled populations had to face. Nomads learned to value various goods they could not produce for themselves, including iron wares, other artisan products, and cereals. Raiding was an unsatisfactory way to supply such wants, since booty seldom matched up accurately with needs, and the risks, even for the most formidable horsemen, were serious. Trading surplus animals and animal products for cereals and artisan goods was an obvious alternative that folded Eurasian and African nomads into the caravan world more and more intimately

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as the centuries passed.[14]

Local protection arrangements for caravan traders were liable to catastrophic reversal whenever raiders and conquerors broke in along the margins of the caravan world. Yet, as familiarity with trade and its conventions spread among the nomadic peoples of Eurasia, new conquerors, wherever they came from and whatever their speech—whether Afghan, Turkish, Mongol, Tibetan, or Tungusic—turned out to be already familiar with and attuned to the rules of caravan management. As a result, disruptions incident to war and conquest became comparatively brief. New rulers regularly welcomed and understood the advantages they could expect from facilitating the resumption of trade.

It took close to a thousand years for such understandings to establish themselves firmly and generally. We know next to nothing about how it happened, other than to observe the propagation of one after another missionary religion along the caravan routes. Still, some landmarks are obvious. Shortly before the Christian Era, caravans began to operate all across Asia, carrying goods between China and Syria along the "Silk Road." Thereafter, contacts were never broken off for long, despite innumerable political and epidemiological disasters. First, Buddhism, Christianity, and Manichaeism spread eastward along the silk route. Later, Islam overtook and largely supplanted these earlier rivals and in the course of time penetrated northward among the Turkish-speaking peoples of the steppe as well. It seems safe to assume that the spread of Islam signified the spread of sympathy with and understanding of the rules of commerce that sustained caravan trade. Among the Mongols, hospitality to all the competing religions of the caravan world had a similar effect. Genghis Khan and his heirs reinsured their worldly success by patronizing multiple paths to the supernatural, whether Muslim, Christian, Buddhist, or shamanistic, and simultaneously supported merchants of any and every kind, as the careers of Marco Polo and other itinerant foreigners demonstrated.

A parallel expansion of the caravan network, followed by conversion to Islam, also took place in Africa both north and south of the Sahara, wherever camels could flourish. Islam's inculcation of attitudes compatible with caravan trade networks had the same effect in the arid regions of northern Africa as in Eurasia.[15] Farther south, a variety of tropical diseases inhibited Muslim penetration. Trade remained slender, and the prevailing form of overland transport continued to be human portage until the late nineteenth century.

North of the desert-oasis zone, where camel caravans predominated, lay the grassy steppelands of Eurasia. Here, camels yielded to horses and mules as the preferred species of pack animal. The reason was partly military: cavalry constituted a more formidable force than camelry because camels will not charge a foe in the way horses can be trained to do. Horses are also swifter for short spurts, a matter of decisive importance in battle maneuvers. Moreover, camel raising was

[14] On nomad dependence on goods from sedentary socities, compare A. M. Khazanov, *Nomads and the Outside World* (Cambridge, 1983), 202–12 and *passim*.

[15] Philip D. Curtin, *Cross-Cultural Trade in World History* (Cambridge, 1984), 15–59.

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a specialized business, and the peoples of the steppe who had learned to build their lives around horses and cattle as early as 2500 B.C. resisted shifting to camels, even though camels carried larger loads and subsisted on poorer forage than horses and mules. But, since grass abounded on the steppe, local preference for horses and mules prevailed.

Horse and mule caravans in the grasslands made connections both with the camel caravan network of the desert south and with the river transport systems of Russia and Siberia. The result, between the thirteenth and fifteenth centuries, was to convert the forests of northern Eurasia into a part of the caravan network. The inclusion of Russia in the Mongol empire signified and symbolized this expansion of the caravan world. The Balkans, where donkeys and mules were the preferred beasts of burden, became another marginal province of that world after the Ottoman conquest, if not before.

It is hard for heirs of the European tradition to appreciate the efficiency of caravans in carrying goods cross-country at a cost that bore comparison with the cost of sea transport for comparable distances. But articles of common consumption like coarse textiles, grain, and metal ingots were in fact transported for scores and sometimes for hundreds of miles on camel back. More valuable goods—silk, spices, incense, gems—traveled even farther of course, but it is a mistake to suppose that these precious items constituted the bulk of caravan cargoes. On trips of 100 to 200 miles or less, food, fuel (especially charcoal), and raw materials like bales of cotton, or leather and timber were far more important, although I must confess that documents from which reliable statistics might be derived do not seem to have been much exploited by historians. This judgment therefore remains impressionistic.[16]

It is clear, however, that, from the seventeenth century, caravan trade between China and the Middle East languished in comparison with European-managed sea trade. Large quantities of goods had begun to go by sea from as early as the eleventh century,[17] but caravan links across Asia nonetheless persisted and may have carried a growing total weight of goods until the mid-fourteenth century or even beyond. After that time, a general decline of steppe population and wealth, resulting, perhaps, from new exposure to bubonic infection, contributed to the decrease in caravan traffic.[18] Political disorders and disruption of irrigation systems in the oases of central Asia had a parallel blighting effect. But, in its Middle Eastern homeland, caravan transportation continued to flourish throughout the eighteenth and into the nineteenth century. Merchants continued to send goods overland from Aleppo to India in the eighteenth century, for example. Only after the opening of the Suez canal in 1869 did caravan traffic across the Gedrosian

[16] My colleague, Halil Inalcik, assures me that documents exist to support exact quantitative studies of caravan transport, and he concurs in thinking that bulk cargoes mattered most for short hauls (personal communication, 1986).

[17] Compare K. N. Chaudhuri, *Trade and Civilization in the Indian Ocean from the Rise of Islam* (Cambridge, 1985), 138.

[18] William H. McNeill, *Plagues and Peoples* (New York, 1976), 149–65.

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desert cease to compete with sea transport between the Levant and India.[19] In the Far East, despite sporadic interruptions by political *contretemps*, caravans carried increasing quantities of goods across the Russian-Chinese border throughout the eighteenth and early nineteenth centuries. German broadcloth and Alaskan furs traveled truly amazing distances to sustain this trade; Chinese tea and silks moved in the opposite direction across equally vast distances, traveling part of the way by caravan and part by boat or sleigh.[20]

As recently as 1944, I witnessed what camels can do, when a DC-3 in which I was flying across Africa landed at Al-Fashir on a desolate air strip in the Anglo-Egyptian Sudan to spend the night. As I sat in the army mess, a string of camels appeared on the horizon, silhouetted against the western sky. The sight was inexpressibly picturesque, but their mission was entirely practical, for each camel had two drums of gasoline strapped across its back. They were refueling the airplanes, bringing gasoline from somewhere in the north—ultimately, perhaps, from the Mediterranean coast—and thereby contributing to their own overthrow as the dominant mode of transport across that desert landscape.

By 1944, of course, caravans were indeed archaic. In the late nineteenth century, acquaintance with the declining decades of caravan trade entered European consciousness as the norm and ceiling for that form of overland transport. Camel and mule caravans had, indeed, become marginal by then, but it is historical myopia to assume that this marginality had always been the case and that wheeled transport always and everywhere was superior to the sparer capital plant and different skills required for caravan management. Instead, we should recognize a millennium, between 300 and 1300 A.D., when caravans carried goods more cheaply, more quickly, and with less seasonal interruption than any other form of overland transport. During that millennium, the superior tonnage that water flotation permitted was not a decisive advantage, partly because shipping was seasonal, liable to shipwreck, and exposed to piracy, but mainly because economic production was not yet attuned to mass exchange of bulk commodities. Without an assured and steady supply, it was extremely risky for a population to become dependent on imported commodities for everyday consumption. Trade in staple goods therefore had to be local; only luxuries could travel across hundreds and thousands of miles. For trade in luxuries, the superior capacity of ships was not of much importance. As a result, for a thousand years and more, animal portage competed successfully with shipping in the movement of luxury goods between China, India, the Middle East, and Eastern Europe.

Western Europe, with its natural riverways, and Mediterranean Europe with its numerous harbors and easily navigated waters, held themselves apart, preserving a transport system that made little use of caravans. Heartfelt antipathy to anything Muslim may have played a part in the west European rejection of caravans. But

[19] Compare Bruce Alan Masters, *The Merchants of Aleppo, 1630–1730: The Commercial and Financial Life of an Ottoman Arab City* (Ph.D. dissertation, University of Chicago, 1982), 82–83.

[20] M. I. Sladkovskii, *History of Economic Relations between Russia and China*, M. Roubley, trans. (Jerusalem, 1966); and Clifford M. Foust, *Muscovite and Mandarin: Russia's Trade with China and Its Setting, 1727–1805* (Chapel Hill, N.C., 1969).

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there were also solid and compelling practical reasons for the failure of caravan transport to spread to the Far West. The agricultural regimen of the open fields made it very expensive to supply animals for any sort of long-distance transport. The three-field system allowed the plow to operate throughout the year, so there was never a season when cultivators had animal power to spare. In addition, fodder was chronically short in winter, so keeping draught animals not required for everyday farming was an extravagance few could afford. Consequently, until the so-called New Husbandry of the sixteenth century introduced fodder crops, and permitted a sharp increase in animal numbers, Western Europe lacked a population of draught animals available for long-distance carriage of goods, whereas further east, a different climate created vast natural pasturelands on which nomads specialized in raising animals suitable for caravan service. Moreover, northwestern Europe's natural river network provided so good a substitute for overland transport that inefficient local haulage over almost impassable roads was an acceptable substitute for the more efficient caravans of the Muslim world. For all its efficiency under propitious conditions, caravan transport confronted inherent limits on its carrying capacity, and, when bulk carriage of coarse goods began to matter in economic life, the caravan world started to suffer from a serious disadvantage. The fact that after about 1200 A.D. first the Chinese and then the Europeans clearly outstripped Muslim technology and wealth registered this fundamental alteration of world relationships.

The basic advantage Chinese and Europeans began to enjoy over the caravan world rested on the elementary nature of water transport: it faced no comparable ceiling on its carrying capacity, and the wind was free. Frequently traveled routes offered no problem, since one vessel's sails do not spoil the wind for another's, except at extremely close quarters. To be sure, river transport required some sort of traction when going upstream, and canal boats also have to be hauled by muscle power. But water flotation allowed even weak human muscles to move far heavier loads than the strongest camel could carry. The problem of finding enough food along the way to fuel the effort of transport by water was therefore much reduced as against the feeding problem that caravans confronted. Special subsidies like those that the Ottoman empire provided for caravans were unnecessary. Market prices could furnish food for animals and men employed in hauling barges and river boats along canals and placid rivers without raising transport costs prohibitively.

China was the first country to exploit the possibility of river haulage on a large scale. China's rivers were not naturally as well suited to water transport as those of Western Europe or Russia, but sufficient human effort devoted to canal building could circumvent even the most formidable floods and rapids. Canals were needed for rice paddy farming of the kind that had established itself in the great Chinese river valleys, beginning about 800 B.C. Consequently, after centuries of labor, the elaborately reticulated canal networks needed for rice cultivation had the additional effect of providing China with a cheap, flexible transport network.

The construction of the Grand Canal (A.D. 605), connecting the Yangtse with the Yellow River, provided China's emerging transport system with what became its

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principal artery, but several centuries passed before canal and river transport transformed Chinese society. The Grand Canal was initially designed to bring taxes in kind (mainly rice) from the Yangtse regions to the court, located always in the Yellow River valley, and to the imperial armies stationed still farther north on guard against attack from nomads. The initial effect of the Grand Canal was that it more than doubled the material resources available to the imperial government and assured China's permanent territorial integration.

The full potential of the Chinese canal transport system was slow to assert itself. Chinese peasants and artisans had first to learn to produce goods for sale in distant markets, and Chinese officials had to learn to tolerate private commerce and pursuit of profit, despite Confucius's condemnation of the obvious immorality of merchants' buying cheap and selling dear without adding anything to the product. Under the Sung Dynasty (960–1279), these changes became widely operative, and the Chinese economy rapidly assumed remarkably modern characteristics. Wealth increased dramatically, as innumerable individuals and social groups started to produce specialized goods for sale at a distance. All the economies of scale and advantages of specialization that Adam Smith was later to describe began to assert themselves.

Confucian officials presided over this commercial enrichment of Chinese society with somewhat embarrassed tolerance. Rather to their surprise, the court, the army, and a new branch, the navy, profited from shifting over to a money economy. In the mid-eleventh century, the government began to levy taxes in money and so became able to meet its material needs by purchasing goods at prices that conformed, more or less, to the market level. More ample and accurately assorted supplies resulted. Government tax policy had the effect of forcing ordinary peasants into the market to get the wherewithal to pay assessed taxes. Everyone had to find something to sell, and many found that, by selling more than was needed to cover tax assessments, they could afford to buy things they could not easily make for themselves.

The market system thus reached downward to the humblest members of society and began to reward technological innovation. Results were spectacular. Chinese productive skills soon outstripped the rest of the world. Poverty did not disappear, for China's population doubled under the Sung, but Marco Polo's awed account of Chinese artisan skills and of the size and magnificence of Chinese cities records a knowledgeable individual's appraisal of China's lead over other Eurasian civilizations in the late thirteenth century.[21]

China's cheap and capacious internal transport system made this remarkable commercial expansion possible. A vast flotilla of canal and river boats, small ones

[21] On China's commercialization, see Robert Hartwell, "Markets, Technology and the Structure of Enterprise in the Development of the Eleventh Century Chinese Iron and Steel Industry," *Journal of Economic History*, 25 (1966): 29–58; Robert Hartwell, "Financial Expertise, Examinations and the Formulation of Economic Policy in Northern Sung China," *Journal of Asian Studies*, 30 (1971): 281–314; and Yoshinobu Shiba, *Commerce and Society in Sung China*, Mark Elvin, trans. (Ann Arbor, Mich., 1970). For some details about the building of China's main canals, see Ch'ao-ting Chi, *Key Economic Areas in Chinese History as Revealed in the Development of Public Works for Water Control*, 2d edn. (New York, 1963), 113 and *passim*.

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hauled by individual boat owners and large ones dragged by gangs of coolies, linked the rich plains of the Yangtse and Yellow river valleys into a single transport network. Small local differences in prices sufficed to set goods in motion, for a canal boat cost little to maintain and yet could carry more than any but the largest caravans. Cheap canal transport allowed bulk commodities to enter the market on a hitherto unimagined scale. Coal and iron as well as rice and other foods flooded into the market. Canal transport was also extraordinarily reliable, since risks of shipwreck were minimal and the overarching imperial government kept brigandage to trivial proportions. It became safe and quite common for rural and urban populations to depend for daily food on imported supplies, while concentrating their own efforts on producing commercial crops or any of an enormous array of goods and services that could be exchanged for cash.[22]

Chinese society, in short, assumed many modern traits under the Sung, and the contagion of Chinese commerce, as it radiated into the steppes via caravans and expanded onto the seas via ocean-going ships, perhaps helped to kindle the spectacular eleventh-century growth of commerce in the Far West. Spices from the East, as everyone knows, constituted a prominent element in Europe's trade between the eleventh and fifteenth centuries, and those spices originated, for the most part, along the coastlands of the Indian Ocean, where the intrusion of Chinese commerce had, from about 1000 A.D., intensified older patterns of production and exchange.

A world-girdling commercial transformation eventually ensued. It was based on cheap sea transport, thanks to improvements in ship design and navigation in which Western Europe took the lead (or perhaps only came abreast of Chinese achievements) by the close of the fifteenth century.[23] But, whatever the initial advantages and disadvantages may have been, European rather than Chinese ships eventually seized control of the world's oceans, and in due season Europeans imposed their updated and technologically improved patterns of transport on the rest of the world. Why China failed to maintain its early lead in ocean-going commerce and exploration is one of the central questions of modern history. Deliberate government policy and ideological commitments on the part of the ruling elite had much to do with the outcome, but, no doubt, deep-seated structural characteristics of Chinese society were also at work.[24]

Thus we see that the Far West's unique combination of river and ocean shipping, supplemented, before the eighteenth century, by expensive and ineffective wheeled traffic, was matched in the Far East by another transport system in which canals and canalized rivers surpassed Western Europe's natural river network and created a much finer reticulation of navigable waterways. This accomplishment reduced overland haulage to comparatively trivial distances and allowed the Chinese to neglect land transport much as Europeans were accustomed to doing.

[22] Yoshinobu Shiba, "Commercialization of Farm Products in the Sung Period," *Acta Asiatica*, 19 (1970): 77–96.

[23] Compare Joseph Needham, *Science and Civilization in China* (Cambridge, 1971), vol. 4, part 3.

[24] Compare Mark Elvin, *The Pattern of the Chinese Past* (Stanford, Calif., 1973).

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But these two systems were eccentric to the ecumene as a whole. Throughout most of that world, caravans sustained a remarkably cheap system of long-distance overland transport, dating back to the early Christian centuries. Its limited carrying capacity became a critical weakness only after about 1300, when cheaper and more reliable shipping began to compete on some long hauls. That limitation became increasingly significant after 1500, when the world's oceans became a single medium for European ships. The caravan world slowly receded thereafter toward marginality until, in our own day, it has almost entirely disappeared. But sound historical perspective requires us to keep its achievements firmly in mind when thinking about the European as well as the Asian and African past. The cost advantages of water-borne transport, of which Edward Fox made so much, applied only to parts of the Old World. Where natural waterways were absent or insufficient, and where natural pastures offered forage free for the taking, or where special subsidies reduced travelers' costs, caravan transport could compete with water transport on even terms and did so for more than a thousand years.

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Crosses and Carets: Renaissance Patronage and Coded Letters of Recommendation

VINCENT ILARDI

IN THE VAST LITERATURE ON RENAISSANCE PATRONAGE, one problem seems to have escaped notice: the existence of letters of recommendation written by patrons on behalf of clients with the expectation that they be disregarded or given little consideration by the recipients. These letters bear all the marks of authenticity, including the autograph signatures of the writers, but they are spurious, because their purpose is to deceive the persons on whose behalf they are written.[1] They are distinguishable from other letters only by the presence or absence of secret signs or codes agreed on in advance by the correspondents. In some instances, patrons wrote letters recommending clients in the highest terms but sent other letters asking recipients not to honor the laudatory ones. More commonly, rulers instructed their ambassadors on the degree of support, if any, to be extended to their recommended clients at the courts of other rulers. All these letters, then, constitute a separate category from that of *pro forma* or perfunctory recommendations, common in any age, which the recipients could readily interpret.

The use of these deceptive practices by Duke Francesco Sforza of Milan and Cardinal Guillaume d'Estouteville of Rouen, two influential patrons in the middle of the fifteenth century, forms the subject of this article. It is not likely, however, that they originated the practice or were the only ones who made use of it. Perhaps

This essay is dedicated to Hans Baron with his gracious permission on the occasion of his eighty-seventh birthday in appreciation for his friendship and wise counsel over the past quarter of a century.

[1] Most books on Renaissance patronage mention letters of recommendation in a cursory manner, if at all. Among the publications consulted are the following: *The Courts of Europe: Politics, Patronage, and Royalty, 1400–1800*, Arthur G. Dickens, ed. (London, 1977); Francis Haskell, *Patrons and Painters: A Study in the Relation between Italian Art and Society in the Age of the Baroque*, rev. edn. (New Haven, Conn., 1980); David S. Chambers, *Patrons and Artists in the Italian Renaissance* (Columbia, Mo., 1971); *Patronage in the Renaissance*, Guy F. Lytle and Stephen Orgel, eds. (Princeton, N.J., 1981); Martin Wackernagel, *The World of the Florentine Renaissance Artist: Projects and Patrons, Workshops and Art Market*, Alison Luchs, trans. (Princeton, N.J., 1981); *Music in Medieval and Early Modern Europe: Patronage, Sources, and Texts*, Ian Fenlon, ed. (Cambridge, 1981); and *Music and Patronage in Sixteenth-Century Mantua*, vol. 1 (Cambridge, 1980). Only Dale Kent, *The Rise of the Medici Faction in Florence, 1426–1434* (Oxford, 1978), 83–104, discusses at length letters of recommendation with regard to form and content, but she makes no mention of deceptive letters, coded or otherwise. Consultations with various colleagues attending the National Conference of the Renaissance Society of America in Philadelphia (20–22 March 1986), where I read an abridged version of this article, as well as correspondence with researchers familiar with letters of recommendation, have convinced me that the system of deception discussed below has never been treated in the literature. I wish to express my gratitude to the following scholars for communicating their views in writing: Creighton Gilbert, Werner L. Gundersheimer, Lewis Lockwood, Edward E. Lowinsky, and Caterina Santoro.

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other researchers can cite additional cases, for it is reasonable to suppose that persons of rank everywhere adopted similar methods to manage a common problem, namely, the flood of requests for letters of recommendation, some of which they were unable or unwilling to deny. Lacking alternative rapid and confidential means of communication such as the telephone, patrons had to devise a secret method to communicate their true intentions without resorting to cipher, which required a key and was more often employed in ambassadorial correspondence. In a real sense, patrons were acting in self-defense, in hopes of having their genuine recommendations honored while avoiding the enmity of disappointed clients. The burden was thus shifted to the recipients, who, presumably, could be expected to cooperate for the sake of friendship and reciprocity. The above suppositions, however, must remain tentative hypotheses until further research establishes whether or not the practice was widespread. It is obvious that their confirmation could lead to a reassessment of the nature and extent of Renaissance patronage.

Researchers who desire to test these hypotheses confront a gigantic task. Letters of recommendation were the lifeblood of Renaissance patronage, as is attested by their survival in massive numbers, especially in Italian repositories, which contain the most extensive collections, at least for the fifteenth century. As would be expected, letters of recommendation are more commonly found in the correspondence with the papacy, the leading source of appointments and favors. This correspondence, in fact, normally constitutes the largest file in Italian archives, and much of it deals with the disposal of church benefices.[2] It is safe to say that in the State Archives of Milan at least half of the Roman correspondence is concerned with clerical appointments in Lombardy and frequently with such appointments elsewhere. If, in addition, we consider internal correspondence, much of which is also concerned with favors and appointments, often in response to letters of recommendation, we can form an idea of the extensive patronage bestowed by leading statesmen of the age. A recent analysis of nearly 1,230 letters addressed to Cosimo de' Medici, filed in the Archivio Mediceo avanti il Principato, has revealed that almost 70 percent include appeals for favors and letters of recommendation.[3] Whether this proportion holds for the entire corpus of Cosimo's correspondence to be found in other repositories in Florence and elsewhere remains to be determined.

[2] I have microfilmed entire files of diplomatic and internal correspondence for the second half of the fifteenth century that are deposited in Italian and other European archives. The films are available on interlibrary loan with the permission of the repositories. A photocopy of the index (66 pages) to the microfilm collection, now consisting of 750,000 frames reproducing some two million documents, can be obtained at cost from the Archives of the University of Massachusetts Library, Amherst. For a summary description of European archival and library collections, see Vincent Ilardi, "Fifteenth-Century Diplomatic Documents in Western European Archives and Libraries (1450–1494)," *Studies in the Renaissance*, 9 (1962): 64–112 (Italian trans. in *Rassegna degli Archivi di Stato*, 28 [1968]: 349–403).

[3] Anthony Molho, "Cosimo de' Medici: Pater Patriae or Padrino?" *Stanford Italian Review*, 1 (1979): 28. Also, a recent study of the correspondence between Mantua and the papal court has found that it consisted largely of letters of recommendation. See Elisabeth Ward Mahnke, "The Political Career of a Condottiere-Prince: Ludovico Gonzaga, 1444–1466" (Ph.D. dissertation, Harvard University, 1974), 211.

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BARELY THREE MONTHS AFTER ASCENDING to the throne, Francesco Sforza revealed to Pope Nicholas V that he was importuned by a great number of requests for letters of recommendation for church benefices in his territory, some of which he was reluctant to deny. Because the appointment of worthy individuals and supporters was important for the consolidation of Sforza's throne, he urged the pope to honor only the letters signed by Sforza's own hand with a cross preceding the signature. The matter was to be kept secret to avoid embarrassment both to the pope and himself.[4]

It is significant that this letter is included in the first register of outgoing ducal correspondence (*Registri delle Missive*), which is almost totally composed of secret instructions to castellans for the period 1447–1458.[5] These instructions reveal that, even before coming to power, Sforza was using a fairly intricate system of secret signs and codes to communicate with officials and others. In addition to the standard password, castellans were instructed to recognize the authenticity of ducal letters by the type and size of seals and the figures represented in them (tree, serpent, woman, old man, and others), the color of the wax (green, red, white), and the signs preceding, following, or intermingled with the duke's autograph signature.

It seems that the practice of coding letters to castellans was carried over to other correspondence, as the following two examples from the same register demonstrate. Genuine letters to a strong supporter of Sforza at the papal court, Cardinal Astorgio Agnesi of Benevento, bore the small seal with green wax and were sent unsigned.[6] Even the dispensing of gifts of velvet cloth was made a state secret because of the great number of such requests and the reluctance to deny them. The duke's signature authorizing these gifts was valid only if it was followed by the word "subscripsit" twice. The responsible officials were admonished to keep the code secret and deal with discontented suitors as best as they could.[7] It is clear that the duke was averse to denying requests for favors, perhaps out of a natural inclination to be liked and also to enhance his prestige as a new ruler by building a reputation as an influential patron ready and willing to dispense favors. Shifting the burden to others served to uphold his reputation but compounded the problem by inviting more requests.

[4] Francesco Sforza to Pope Nicholas V, Lodi, 18 June 1450, Archivio di Stato, Milan, Archivio Sforzesco Ducale, *Registri delle Missive* (hereafter, *RM*), 1, fols. 131v–32. (Henceforth, documents cited without archival designation are understood to be located in this archive.) This letter was published in a mimeographed edition of *RM*, 1, prepared by Gabriella C. Poli and Carlo Paganini, vol. 1 (Milan, 1981), 109–11. Because of the limited circulation of this work, I am providing the letter in the Appendix, no. I, with minor changes necessitated by my reading of the original on microfilm.

[5] This register is in fact titled, "Ordini di Francesco I ai castellani," in *Inventario*, no. 5, but it also contains letters to others as well as some letters received. For additional examples of instructions to castellans with the complex coding system described below, see *Registri Ducali*, 170 (1466–79) and 171 (1466–81).

[6] Chancery notation describing this code, Lodi, 24 June 1450, *RM*, 1, fol. 133 (vol. 1, 111). The fact that this notation is crossed out, however, may mean that the code was not put into effect or that another code, registered elsewhere, was to be used. For another crossed-out notation about coded correspondence with Niccolò Amidano, Bishop of Piacenza, 9 May 1449, see *RM*, 1, fol. 131 (vol. 1, 109).

[7] Sforza to Ser Antonio Longo and Matteo da Pesaro, Lodi, 31 January 1451, *RM*, 1, fol. 68 (vol. 1, 45–46).

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Sforza was not alone in facing this dilemma. Three years after the duke wrote the above letter to Pope Nicholas, Cardinal Guillaume d'Estouteville asked Sforza to recommend to the pope a scribe at the papal curia for a church benefice in Lombardy. At the same time, the cardinal confided that many of his clients were requesting letters of recommendation to the Milanese court, and, being unwilling to deny them, Guillaume urged the duke to honor only the recommendations with his autograph signature followed by a caret-shaped sign.[8]

The duke's reply has not been found, but we can surmise that the request was granted by the fact that the letter was registered for ready reference and by the survival of subsequent, similarly coded letters addressed to Sforza.[9] The cardinal was a relative of Charles VII and had used his influence to support Milan's efforts for an alliance with the king while serving as a papal legate in France (1451–1452).[10] He was also one of the richest and most influential members of the college of cardinals, living in a style suitable to a secular prince with a reputation as a generous patron. I have not been able to establish whether he developed the coding practice independently, or learned it in Rome, where he preferred to reside, or elsewhere. It is known that he employed Italian secretaries who wrote his letters in a clear, humanistic hand, and that one of the secretaries thought that his master "was more Italian than French."[11]

Likewise, we lack Nicholas's response to the duke's request, and only one letter[12] addressed to the pope with the special sign has been found in Milan, seemingly because the originals have not survived at the Vatican Archives. There is little doubt, however, that the pope cooperated, because on 1 April 1450 he had already issued a bull sanctioning a *de facto* prerogative already enjoyed by the Visconti rulers, which allowed the duke to present candidates for appointment to vacant

[8] Guillaume d'Estouteville to Sforza, Rome, 30 June 1453 (orig.), *Potenze Estere* (hereafter, *PE*)–Roma, cart. 40 (see Illustration 1); copy in *RM*, 1, fol. 120v (vol. 1, 95–96). The copy lacks the day and month. It should be noted, however, that some of his earlier letters of recommendation addressed to Sforza from Rome (cart. 40, 5 February, 20 March 1453) bear the same sign. It may be that the June letter was a reminder or that the sign simply served to authenticate the signature in important letters. The practice of authenticating signatures by adding flourishes or signs was common at this time.

[9] The following letters of recommendation, addressed to the duke from Rome in 1453, bear the secret sign: 17 September, 28 October, 8 and 21 November (*PE*–Roma, cart. 40). Others, bearing the cardinal's autograph signature, do not.

[10] For biographical information on the cardinal and his mission to Charles VII, see Ludovico von Pastor, *Storia dei Papi dalla fine del Medio Evo*, trans. Angelo Mercati, 20 vols. (Rome, 1958), 1:370, 457–61; and Gaston du Fresne de Beaucourt, *Histoire de Charles VII*, 6 vols. (Paris, 1890), 5:191–200. For his role in the alliance, see Luigi Rossi, "Lega tra il Duca di Milano, i fiorentini e Carlo VII, Re di Francia (21 febbraio 1452)," *Archivio storico lombardo*, ser. IV, 5 (1906): 258–61, 277–78, 288–89.

[11] The cardinal's Italophilia is attested by his secretary, Giovan Battista degli Artizagani da Cremona, who wrote to Cicco Simonetta, Sforza's chief secretary, as follows: "Io non poria dire la summa affectione che ha Monsignore a le parti de Italia e corte romana, perchè quantunche qui sia fra li soi, che sono di sangue regale, et riceva grandissimi honori, pur nientedimeno nullo altro singular desiderio ha che di pensare lo suo retorno in Italia dove a lui pare consista ogni suo vivere e felicitade . . . conosco lui essere più italiano che francese." (Bituri [Berry], 16 June 1452, Bibliothèque Nationale, Paris, *Fonds Italien*, Cod. 1586, fol. 133.)

[12] Dated from Gambara, 5 December 1452, this original letter actually has a cross preceding and one following the duke's signature (*PE*–Roma, cart. 40). It recommends a candidate for a vacant canonry in Lodi and it asks the pope to lend credence to what the Milanese envoy, Nicodemo Tranchedini, was instructed to say on this subject. Presumably, the letter was returned by Tranchedini.

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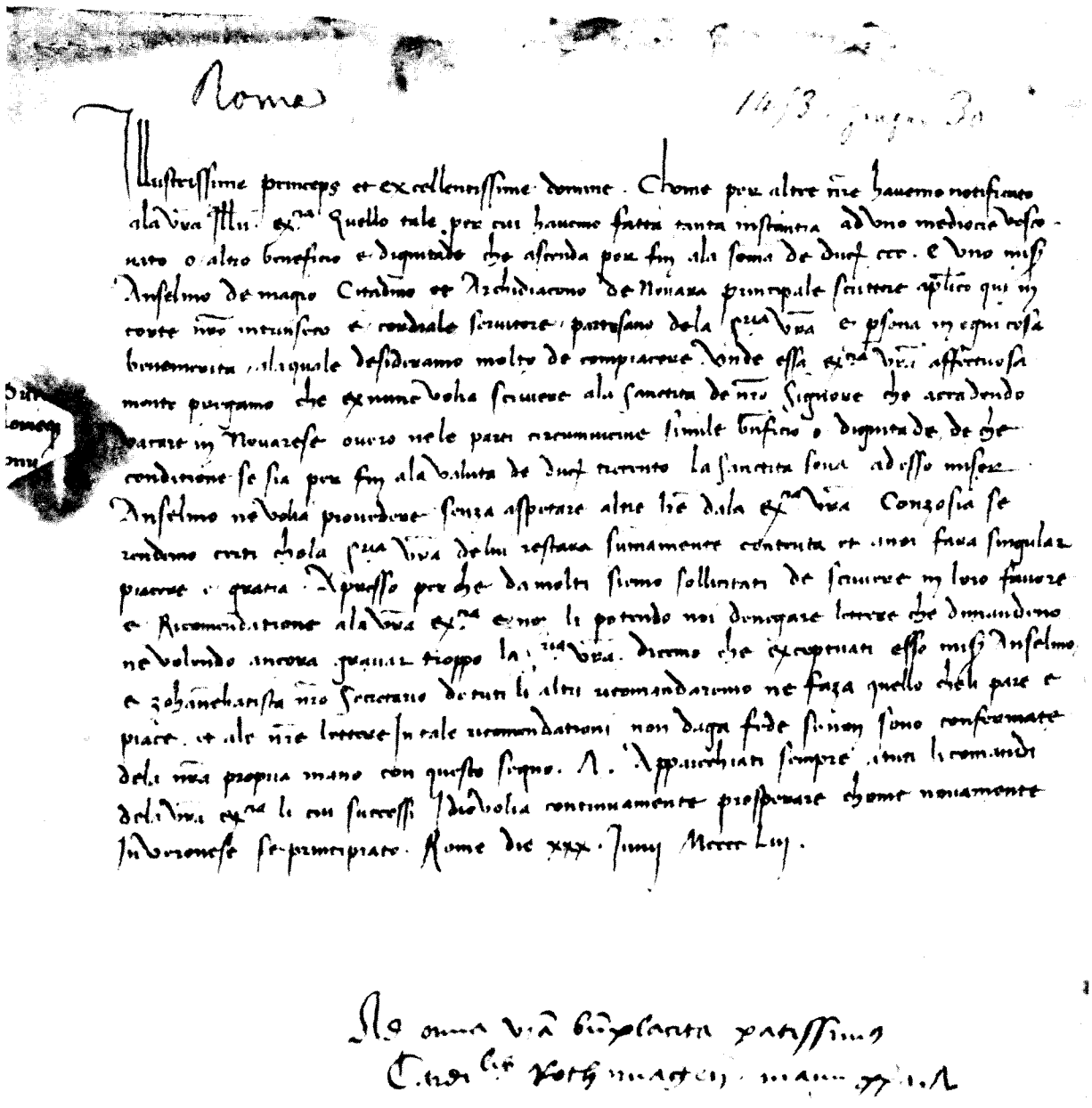


ILLUSTRATION 1: Letter of Cardinal Guillaume d'Estouteville of Rouen to Duke Francesco Sforza of Milan, Rome, 30 June 1453 (original), Archivio di Stato, Milan, Archivio Sforzesco Ducale, *Potenze Estere*—Roma, cartella 40. Note autograph signature followed by caret-shaped code.

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benefices in his territory.[13] Sforza's letter, then, demonstrates his resolve to take advantage of the privilege, and build support for his new regime through his largesse without creating enemies at a time when Venice, Naples, and Savoy were making preparations to contest his succession by force.[14] It is also probable that he was already collecting fees from the candidates to replenish his empty coffers, an accusation later made against him.

Nicholas's successor, however, refused to confirm the privilege, claiming that it was opposed by the college of cardinals and that Nicholas himself had regretted having granted it. But, as Calixtus III became increasingly dependent on Milan's support in his struggle to dislodge the mercenary adventurer, Giacomo Piccinino, from papal territory, and contain King Alfonso of Naples' aggressive moves in Italy, he modified his stand to the point of accepting most of Sforza's nominees. The process nevertheless now required protracted negotiations that taxed the duke's patience and diplomatic ability, especially because Calixtus insisted on reserving some of the Lombard sees for his nephews, Rodrigo and Pier Luigi Borgia.[15] At this point, it became obvious that the sign system previously used to recommend ducal nominees had to be buttressed by representations made by a resident ambassador.

In June, 1455, the duke sent a jurist, Giacomo Calcaterra, to take up residence at the papal court and conduct the delicate negotiations that hitherto had been handled by a number of special ambassadors, including Alberico Maletta, Bishop Bartolomeo Visconti of Novara, and Nicodemo Tranchedini, the last shuttling from his regular post in Florence.[16] At the end of August, Calcaterra reported that he was frequently asked for assistance at the papal court by various persons bearing ducal letters of recommendation. Knowing that such letters were often granted in order to end pestering by persistent suitors, he requested secret signs revealing his master's true wishes so that he would know how to proceed and could spare the pope and the cardinals much annoyance.[17] This dispatch indicates that far too many letters of recommendation were flowing from the Milanese court and that

[13] Luigi Fumi, "Chiesa e Stato nel dominio di Francesco I Sforza," *Archivio storico lombardo*, 51 (1924): 5–6; and Luigi Prosdocimi, *Il Diritto ecclesiastico dello Stato di Milano dall'inizio della Signoria viscontea al periodo tridentino (sec. XIII–XVI)* (Milan, 1941), 64–66.

[14] Sforza's precarious position in 1450–51 has been amply illustrated by Luigi Rossi, "Venezia e il re di Napoli, Firenze e Francesco Sforza dal 1450 al 1451," *Nuovo archivio veneto*, new ser., 10 (1905): 5–46, 281–356.

[15] Sforza's relations with Nicholas and Calixtus with respect to church benefices have been treated in some detail by Fumi, "Chiesa e Stato," 8–32. Fumi, a former director of the State Archives in Milan with a profound knowledge of the documents, missed these coded letters, perhaps because another archivist, Beno della Croce, selected the documents for him after Fumi retired (p. 74). They also escaped the attention of Marina Valori, "Alcuni cenni sui rapporti tra Francesco I Sforza e papa Niccolò V e Francesco II Sforza e papa Clemente VII," in *Squarci d'Archivio Sforzesco* (Milan, 1981), 72–78.

[16] Much of the reporting of the three special ambassadors, however, was related to the negotiations surrounding the formation of the Italian League in 1455, but a good deal was devoted to the disposal of benefices in Lombardy. On 18 June 1455, Sforza issued instructions at Cremona to Bishop Giovanni Barbavara of Tortona, Alberico Maletta, Tommaso da Rieti, and Calcaterra to congratulate Calixtus on his election, pledge obedience in his name, and treat various affairs of state including benefices (Biblioteca Ambrosiana, Milan, Cod. Z219 Sup., no. 9180; and *PE*–Roma, cart. 41).

[17] Giacomo Calcaterra to Sforza, Rome, 31 August 1455, *PE*–Roma, cart. 42 (see Appendix, no. II).

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frequently they were carried by the petitioners themselves, who had full knowledge of their contents.

The duke, who had just written to Calcaterra that his good relations with the pope were eliciting still more requests for recommendations,[18] finally sent the ambassador the following instructions on the relative worth of his letters so as to avoid confusion and eventual errors in interpretation. He specified that instructions about affairs of state must be obeyed and required no sign. Letters dealing with the affairs of others (letters of recommendation) henceforth were to be divided into three categories, coded as follows: those bearing the duke's signature followed by "SSS" were to be fully supported; those with "SS" after the signature were to receive less support; unsigned letters were to receive the least support.[19] This coding system made clever use of the common abbreviation for "subscripsi" or "subscripsit," which was far less obvious than the cross before the signature, but left Calcaterra with the onerous task of assuaging the feelings of the recommended candidates in the unsigned category.

The same coding system was continued with the new resident ambassador, Ottone del Carretto, who assumed his duties in December, 1456. Calcaterra had drowned in an accident three months earlier, together with four members of his household and some of his papers, while crossing the Tiber.[20] In the three-year period from the initiation of the new system to the election of Pope Pius II in August of 1458, fifty-two original letters of recommendation addressed to Calcaterra and Ottone bear the double "S" code, while about twice that number were signed only by the duke's secretaries, chiefly Cicco Simonetta and his brother, Giovanni.[21] Only two letters were assigned the triple "S" code. But these figures are incomplete; not all original letters have survived, ambassadors did not return some letters to the chancery despite instructions to do so, and a portion of the letters may have perished with Calcaterra. In addition, a few letters of recommendation are addressed directly to the popes, and some recommendations were included in ducal instructions dealing with state affairs. Finally, evidence exists of inconsistency at the Milanese chancery—despite its reputation for organization and efficiency—because some of the letters dealing with key appointments, which were discussed in detail in the ducal instructions, were not placed in the first category. Only a painstaking analysis of instructions and dispatches dealing with church appointments can hope to achieve more complete and accurate results.

[18] "Sentendendo [*sic*] li nostri subditi la Sanctità de nostro Signore essere verso nuy ben disposta, credeno pur le nostre littere et recomendacione valere assay presso Soa Sanctità et perhò recoreno da nuy, rechiedendone gli recomendamo alla prelibata S.tà de nostro Sig.re, chi per una cosa chi per un'altra"; Sforza to Calcaterra, Milan, 27 August 1455, *PE*–Roma, cart. 42.

[19] Sforza to Calcaterra, Milan, 16 September 1455, *PE*–Roma, cart. 42, see Appendix, no. III and Illustration 2.

[20] Calcaterra's last dispatch was written from Castelgiubileo, 7 September 1456 (*PE*–Roma, cart. 44). He was planning to return to Rome after his recovery from the plague that he contracted there. He drowned crossing the river on his return trip (Bishop of Novara to Sforza, from the baths near Viterbo, 7 October 1456, cart. 44). Ottone's instructions (Pavia, 23 November 1456, cart. 44) do not mention the coding system, but he most certainly was briefed orally.

[21] These letters were found in *PE*–Roma, cart. 42–47 and in the Biblioteca Ambrosiana, Cod. Z219 Sup. Of the fifty-two letters, only four were addressed to Calcaterra.

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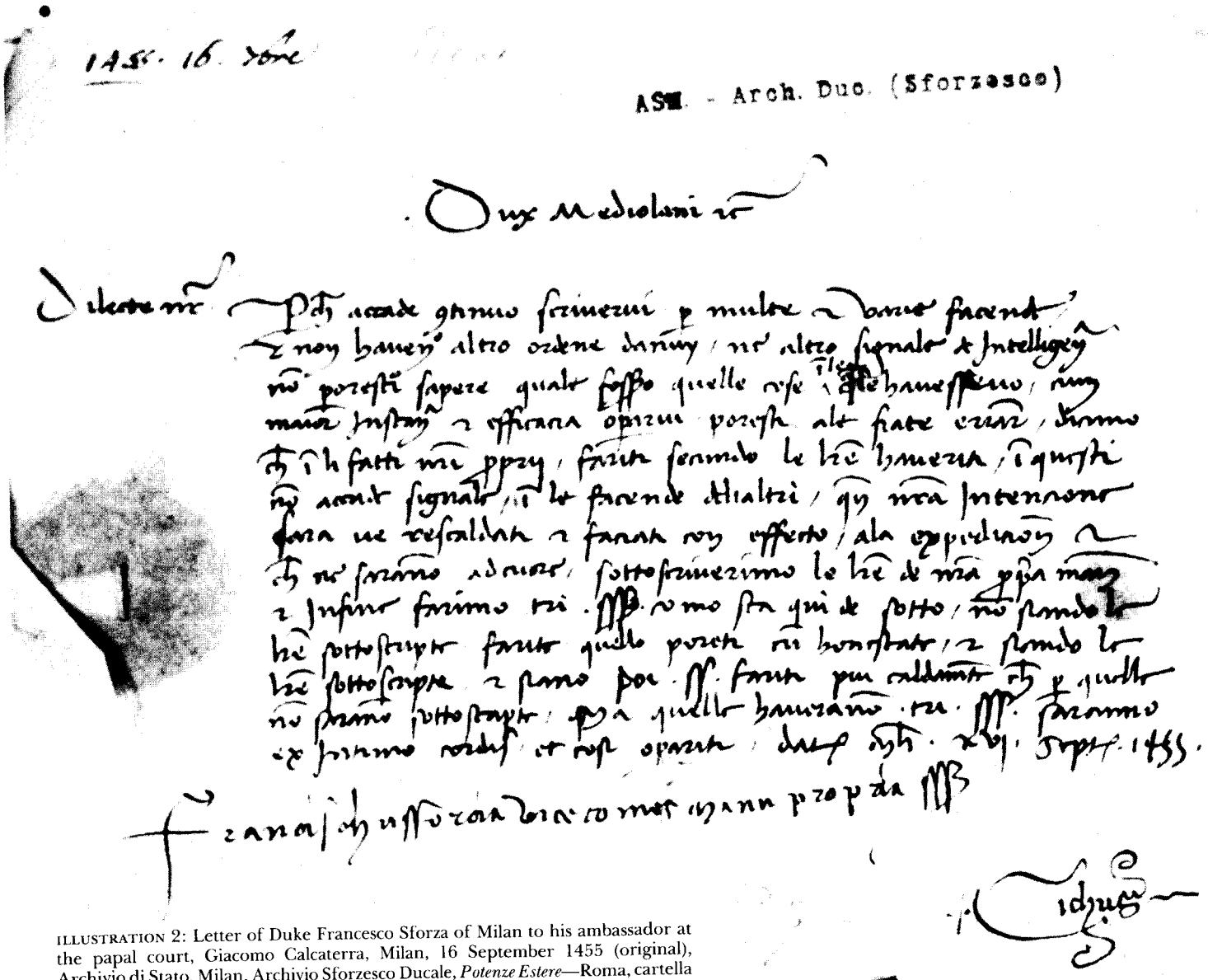


ILLUSTRATION 2: Letter of Duke Francesco Sforza of Milan to his ambassador at the papal court, Giacomo Calcaterra, Milan, 16 September 1455 (original), Archivio di Stato, Milan, Archivio Sforzesco Ducale, *Potenze Estere*—Roma, cartella 42. Note autograph signature followed by "SSS" code. Countersigned by his chief secretary, Cicco [Simonetta]. See Appendix, no. III, for transcription.

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The above figures nevertheless give an approximate indication of the extent of Sforza's real or feigned patronage at the papal court, which led to charges that he was selling benefices and that he kept an ambassador in Rome primarily to press for appointments, contrary to diplomatic custom.[22]

It is significant that the two triple "S" letters recommended the promotion of Milan's candidates for the college of cardinals during Calixtus's pontificate. The competition for the red hat had always been intense among rulers, who wanted to be represented in the college and gain a voice in papal elections. Their wishes were often frustrated by the desire of the cardinals to uphold their prestige through limiting their number and by the nepotism of the popes themselves. These contrasting forces clashed in the two consistories called by Calixtus for the creation of new cardinals. King Alfonso of Naples recommended seven candidates and threatened reprisals if his wishes were not satisfied.[23] Sforza, a much more moderate and prudent ruler, recommended three, on the grounds that Lombardy had usually been represented by two or three cardinals but now had none. A special ambassador, Giovanni Caimi, was sent to Rome in 1456 to press the matter, and two triple "S" letters went out listing the candidates in order of preference: Bartolomeo Visconti (Bishop of Novara), Giovanni Castiglioni (Bishop of Pavia), and Giacomo Antonio della Torre (Bishop of Modena).[24] When it became clear that the cardinals would not consent to Visconti's elevation because of his reputation as an overly ambitious adventurer, the pope promoted the Bishop of Pavia in December of 1456. Visconti died the following April.[25]

Recommendations for the cardinalate at times required delicate and highly secret negotiations because they clashed with those of friends and allies competing

[22] Nettled by Calixtus's complaint that the duke was selling benefices, Ottone communicated his embarrassment to Cicco Simonetta: "Almeno mi pare seria bene che quando de beneficii se prenda denari, se fecesse più secreto sichè qua non ne venesse fama et io dicesse al nostro S. che è deliberato il nostro Ill.mo S.re de lassar tal usanza"; Rome, 24 May 1457, *PE*–Roma, cart. 45. Some members of the predominant Catalan faction at Calixtus's court, which wanted complete control of all benefices, reminded Ottone that "non è officio de oratore expedire similia [benefices], ma solo attendere a le cose de Stato"; Ottone to Sforza, Rome, 5 January 1457, Biblioteca Ambrosiana, Cod. Z219 Sup., no. 9285.

[23] Sceva de Curte to Sforza, Rome, 22 February 1456, *PE*–Roma, cart. 43.

[24] Of the two triple "S" letters, both of 28 April 1456, one was addressed to Sceva de Curte, Calcaterra, and Orfeo de Ricavo and the other to Calcaterra alone. In the latter, Calcaterra was instructed that, once he had secured Visconti's promotion, he should press for the elevation of the bishops of Pavia and Modena on equal terms (*PE*–Roma, cart. 43). In another double "S" letter to the same ambassador (3 May 1456, cart. 44), Sforza revealed that the Bishop of Pavia had complained that his promotion had not been fully supported. Calcaterra was to reassure him on this point but still work for Visconti's promotion first and give G. A. della Torre the least support. Of course, della Torre was to receive similar assurances. Castiglioni went as far as to dispatch to Milan his own draft of a letter supporting his elevation, which he urged the duke to sign as quickly as possible, warning that "questa è una cosa che non se fa se una volta" ("this is a matter that can be done only once"); Castiglioni to C. Simonetta, Rome, 4 January 1456, cart. 43. For his part, the Bishop of Modena complained bitterly to C. Simonetta (Rome, 22 December 1455, cart. 43) that Giovanni Caimi had acted contrary to the duke's instructions in not sufficiently exerting himself for his promotion. This episode illustrates once more the plight of ambassadors acting under secret instructions.

[25] While Castiglioni was popular among the cardinals, Visconti was thoroughly detested. Some of them even declared that they "buttariano più tosto il suo capello in la merda" ("would rather throw their hats in a dung pile") than accept Visconti's promotion (Caimi to C. Simonetta, Rome, 4 February 1456, *PE*–Roma, cart. 43). For additional details on the two consistories, see Fumi, "Chiesa e Stato," 13–20; and *Pastor, Storia dei Papi*, 1: 752–58. Like Fumi, Pastor was not aware of the Milanese coding system.

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for the limited number of openings in the college. On these occasions, letters of recommendation were supplemented by messages delivered by special emissaries, operating in strict secrecy without even the knowledge of resident ambassadors. Such a mission was undertaken by Angelo Acciaioli, a member of Cosimo de' Medici's inner ruling circle and a friend and sometime resident adviser of Francesco Sforza. In October, 1460, the duke instructed him to go to Florence and solicit an invitation for an audience with Pius II. He was to inform Cosimo and the Milanese resident in Florence, Nicodemo Tranchedini, that he had received and accepted the invitation without Sforza's knowledge—a claim not easily supportable had the mission proceeded directly from Milan. Once in Rome, Acciaioli was to make clear to the pope that, although the duke supported the promotion of Cosimo's nephew, Filippo de' Medici, Bishop of Arezzo, and of Francesco Gonzaga, son of the Marquis of Mantua, he would prefer to have his own men in the college, namely, Iacopo Ammannati, Bishop of Pavia and a friend of the pope, and the protonotary, Stefano da Forlí. Acciaioli was to point out that their promotion would be of signal value in the preservation of the Sforza dynasty, which required a friendly pope and some cardinals wholly dependent on the duke.[26]

The pope promised to take into account the duke's wishes in the next round of promotions and pointed out enigmatically to Acciaioli that the Florentines themselves were not as friendly towards Sforza as Pius had once thought.[27] (Was Cosimo playing the same game?) At the same time, Ottone, unaware of Acciaioli's message, was strongly supporting Filippo de' Medici, especially because he had received a copy of a ducal letter addressed to the pope warmly recommending his promotion. Feigning surprise over this unusually strong pressure, Pius kept the secret and simply informed the ambassador that Filippo's unsavory reputation, his pro-French views, and the direction of his uncle's foreign policy did not make him a strong contender.[28] The following year, the pope elevated only Gonzaga and

[26] The secrecy of this mission is further underlined by the fact that Acciaioli himself wrote the instructions (Milan, 28 October 1460, Biblioteca Ambrosiana, Cod. Z219 Sup., no. 9244). In an autograph letter addressed to Pius and carried by Acciaioli (Milan, 21 October 1460, *PE*–Roma, cart. 48), Sforza made this eloquent appeal for the success of the mission: "Da poy la salute de l'anima, non è cosa che pur me astringa et prema el cuore che ordinare questi miei figlioli che possano da poy me conservare in pace questo Stato . . . Il perchè, per questa sola cagione, mando da la S.tà Vostra il Magnifico Cavaliero Misser Angelo Azzaiolo . . . il quale non mandaria se questo facto non me stringessi quanto stringe. Prego adonque et supplico humilmente et devotamente la B. V. che la se degni benignamente intenderlo et exaudirlo et consolare me et li mei figlioli de tale gratia, la quale haveremo et teneremo in eterna memoria." For a brief account of Acciaioli's relations with Sforza, see *Dispatches with Related Documents of Milanese Ambassadors in France and Burgundy, 1450–1483*, vol. 1, 1450–1460, Paul M. Kendall and Vincent Ilardi, eds. (Athens, Ohio, 1970), 1: xli–xliii.

[27] Acciaioli arrived in Rome on the evening of 25 November (Acciaioli to C. Simonetta, Rome, 26 November 1460, *PE*–Roma, cart. 48). He reported the results of his first audience with the pope in his dispatch to Sforza of 1 December (cart. 48). Other dispatches about subsequent audiences have not been found, but it is likely that he gave his final report orally. He planned to leave Rome on 9 December (Acciaioli to Sforza, Rome, 8 December 1460, cart. 48).

[28] Specifically, the pope complained that Cosimo had not fully supported his policy of helping King Ferrante of Naples expel the invading army of Duke John of Anjou, and had delayed the collection of church tithes in Florentine territory to finance his projected crusade against the Turks (Ottone to Sforza, Rome, 15 December 1460, *PE*–Roma, cart. 48).

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Ammannati.[29] The duke now had a second man in the college, but he had taken the risk of tarnishing his credibility (if exposed) in the eyes of two of his closest allies: the banker, who regularly propped up his shaky finances, and the ruler of a client-state, who for several years had served Milan as its most prestigious and faithful *condottiere*.[30]

It was equally vital to keep one's known or potential enemies out of the college of cardinals and avoid the risk of having to deal later with an unfriendly pope. Such an opportunity presented itself soon after Pius's election, when Guglielmo of Monferrat asked the duke to recommend his brother, Protonotary Teodoro of Monferrat, to the pope. This request created a dilemma at the Milanese court. On the one hand, Guglielmo was one of Sforza's leading *condottieri*, whose allegiance was worth cultivating especially because he was slated to succeed his brother, Giovanni, on the throne of that strategic state. He was also an ambitious and haughty general who did not gladly suffer slights.[31] On the other hand, Teodoro's well-known French sympathies clashed with the goals of Milanese foreign policy, which sought to prevent the French monarchy from gaining a foothold anywhere in the Italian peninsula.[32] The elevation of such a man, who might conceivably become pope, would have been a serious setback for Milan. It was a delicate matter requiring extraordinary measures.

Sforza rose to the occasion in his typical fashion. He wrote letters in his own hand to Pius recommending Teodoro in the strongest terms and sent instructions to his ambassadors to exert themselves for Teodoro's promotion.[33] Two special envoys, Sceva de Curte and Giovanni Caimi, were dispatched to plead the case. At the same time, the duke sent ciphered instructions to Ottone with orders to commit them

[29] Pastor, *Storia dei Papi*, 2 (Rome, 1961), 197–99. There was strong opposition in the college to the creation of new members. Pius himself gave a vivid account of this struggle in his *Commentaries*, Book VII, trans. Florence A. Gragg, ed. Leona C. Gabel, *Smith College Studies in History*, 35 (1951): 496–504.

[30] The political and financial ties binding Cosimo and Sforza are treated in detail in my article, "The Banker-Statesman and the Condottiere-Prince: Cosimo de' Medici and Francesco Sforza, 1450–1464," to be published in the proceedings of the Seminar, "Florence and Milan: Comparisons and Relations," held at Villa I Tatti in September 1984. It is also included in my *Studies in Italian Renaissance Diplomatic History* (London, 1986), no. 4. For the intimate relations between Ludovico Gonzaga and Sforza, see Mahnke, "The Political Career of a Condottiere-Prince," 111–204, 301–84. In relating the efforts made for Gonzaga's promotion in Rome and elsewhere, particularly in Germany, Mahnke pointed out that no supporting letters were sent by Milan (264–65).

[31] Guglielmo had joined the anti-Sforza coalition during the war for the Milanese succession in the hope of regaining some contested border lands previously taken by Milan and had only recently settled his differences with the duke. On their relations, see Maria Damarco, "Guglielmo I Paleologo (Marchese di Monferrato, 1420–83)," *Rivista di storia, arte e archeologia per le provincie di Alessandria e Asti*, 42 (1933): 541–63; and Orsolamalia Biandrà di Reaglie, "Ricerche sui rapporti tra il Monferrato e Milano nel secolo XV," *ibid.*, 92 (1973): 61–66.

[32] See Vincent Ilardi, "The Italian League, Francesco Sforza, and Charles VII (1454–1461)," *Studies in the Renaissance*, 6 (1959): 151–53, for Sforza's persistent efforts to oppose French territorial ambitions in Italy.

[33] In addition to the duke's autograph letter mentioned in his instructions to Ottone (cited in n. 34), two years later, he sent another to Pius (28 November 1460, Biblioteca Ambrosiana, Cod. Z219 Sup., no. 9245). See also his double "S" letter to Sceva de Curte and Ottone (19 November 1458, *ibid.*, no. 9478) in which he instructed them to work mightily for Teodoro's promotion: "perchè essendo luy [Teodoro] quello prelato ch'el è et essendo tra nuy et quilli Signori de Monferrato quello intrinseco amore, benivolencia et carità che sapeti, haveremo tanto caro ogni honore et bene che sarà facto a la Signoria Soa quanto s'el fosse facto alla persona nostra propria." Similar letters written to the same ambassadors on 29 November and 4 December 1458 are in *PE*–Roma, cart. 47.

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to memory and burn them immediately. The instructions contained the following message for the pope's ears only. Sforza first excused himself for having been constrained to write many letters of recommendation on behalf of petitioners who wished to take advantage of his good relations with the Holy See. He then reminded the pope that his true wishes in these matters would always be revealed to Ottone by means of appropriate instructions and signs. A case in point was the enclosed letter he had been pressed to write in his own hand to the pope by Guglielmo in favor of Teodoro. Sforza announced that he was sending Sceva de Curte, who would speak about this promotion more warmly than he had been instructed because the ambassador was a friend of the Monferrats. In view of the fact that it would be contrary to Milan's interests to have a cardinal in the college who, if he became pope, would outdo a Frenchman in his zeal for France, he begged Pius to disregard his letter and Sceva's pleadings, and refrain from promoting the protonotary. But this request was a secret that the pope should keep to himself.[34]

Pius, who shared Sforza's anti-French sentiments, cooperated fully and even expressed admiration for the way the matter had been handled in Milan.[35] An experienced and astute diplomat, Pius informed the Milanese ambassadors that he liked Teodoro and would certainly keep him in mind at the appropriate time, but he cautioned that it was sometimes easier to gain the cardinals' assent for candidates not of noble rank.[36] In communicating this deceptively hopeful response to the Monferrats, Ottone carried the charade a step further by inviting Teodoro to come to Rome and facilitate his candidacy by his presence.[37] These intricate practices of deception illustrate the problems faced by historians of Renaissance patronage. On the basis of all this ambassadorial activity and related correspondence, scholars would naturally conclude that the protonotary's promotion was eagerly sought by the duke of Milan and desired by the pope. This

[34] Sforza to Ottone, Milan, 21 October 1458 (minute), Biblioteca Ambrosiana, Cod. Z219 Sup., no. 9476: "scriveremo sempre a voy, quale intenderite l'animo nostro secundo li signali et ordini havete da noy et lo direti a la S.tà Soa . . . E perchè per quanto noy consideramo non se fa per noy che dicto prothonotario havesse tale dignitate che potesse venire ad papato, che saria pegio et più caldo per la parte de Franza che non saria uno franzoso et per più altri respecti, Misser Sceva, per passione de amicitia ha ad quella Casa, dirà più caldamente che non gli havemo dicto noy." Therefore Ottone was to inform the Pope "che non intendiamo nè per quello gli haverà dicto Misser Sceva nè anchora per quella nostra littera de nostra mano gravare la Soa S.tà de tale facenda, ymmo haveremo caro et così pregamo la Soa S.tà non lo faza per multi respecti; ma questo secreto se degni tenere nel pecto suo et non lo manifestare ad altri . . . Et questa littera non veda altri che voy: dezifrata et lecta, la buttati nel fuoco . . . Dopo scritta questa, el prefato S. Guielmo ha voluto ve scrivamo una littera a voy et un'altra al dicto Misser Sceva, sichè ve ne avisamo."

[35] "È molto piaciuto alla S.tà de N. S. el modo havete servato in scrivere per lo facto del prothonotario de Monferà, la qual cosa ha ben intesa"; Ottone to Sforza, 15 December 1460, *PE*–Roma, cart. 49. This letter, written in reply to the second autograph letter sent by the duke in 1460 (cited in n. 33), shows that the charade continued for some time. It is significant that the pope made the above comment right after he expressed reservations about the promotion of Filippo de' Medici.

[36] Ottone and Sceva de Curte to Sforza, Rome, 11 November 1458, and G. Caimi to Sforza, Rome, 20 December 1460, *PE*–Roma, cart. 47 and 49, respectively.

[37] Ottone to Teodoro and Guglielmo of Monferrat, Rome, 14 November 1458, *PE*–Roma, cart. 47. The effort was in vain, although ultimately, Teodoro was promoted in 1467 by Pope Paul II after Sforza's death.

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conclusion would be justified were it not for the chance survival of the secret instructions sent to Ottone.

PIUS'S ENDORSEMENT OF THE DUKE'S METHODS might have been less enthusiastic had he known that they had been applied to him just before the conclave that resulted in his election. When Calixtus became gravely ill at the end of July, 1458, Sforza received an urgent dispatch from Ottone, advising him to act quickly in naming his favorite candidate because the conclave was scheduled to meet within ten days of the pope's expected death. The ambassador recommended an ardent supporter of Sforza, Domenico Capranica, Cardinal of Fermo, who enjoyed wide support in the college. Only the vote of Giovanni Castiglioni, Cardinal of Pavia, was needed to ensure success. Although Castiglioni seemed to be favorably disposed, Capranica himself suggested that Sforza make the cardinal's cooperation more certain by writing Castiglioni a letter in his own hand in which he expressed support for Castiglioni's candidacy but at the same time pressed him to vote for Capranica once Castiglioni had lost hope of winning. This letter was to be read by Castiglioni in Ottone's presence and then destroyed.[38]

The duke implemented the suggested strategy with some important tactical modifications. The following ciphered instructions (coded "SSS") were sent to Ottone with orders to keep them secret. The ambassador was to use all possible diligence to secure the election of Capranica, whom the duke loved as a second father, as an enclosed letter addressed to Capranica amply demonstrated. He was also enclosing letters addressed to Ottone and Castiglioni, with which he pretended to support Castiglioni.[39] At the same time, Ottone was to urge Castiglioni to give his vote to Capranica at the appropriate time, that is, when he had lost hope of being elected himself. But, if the ambassador were to ascertain that Capranica would not be chosen, then he should support Cardinal Prospero Colonna and urge both Capranica and Castiglioni to cast their votes for him. Finally, if he felt that Colonna had no chance of success, he should back any other candidate suggested by Capranica, who was to be kept fully informed of the duke's wishes because he could be trusted to act in Milan's best interests. In any case, care should be taken that the successful candidate knew he had been fully supported by the duke. To achieve this objective, the duke was enclosing another letter coded with a cross

[38] Ottone to Sforza, Rome, 26 July 1458, *PE*–Roma, cart. 47, published by Ludwig von Pastor, *Ungedruckte Akten zur Geschichte der Päpste vornehmlich im XV., XVI., und XVII. Jahrhundert*, vol. 1, *1376–1464* (Freiburg, 1904), 80–82. This dispatch demonstrates that Capranica, who was generally admired for his piety, charity, modesty, and learning, actively sought the papal tiara and was not above using intrigue to obtain it. It is surprising that Pastor did not take sufficient account of it to temper his portrait of the cardinal, whom he described as a saintly figure (*Storia dei Papi*, 1: 776–86). Pius, who had been Capranica's secretary, was perhaps nearer to the truth when he wrote that Capranica had "*too passionately* aspired to succeed" Calixtus and that "he was a man who would have been a model of virtue, if he had not let *ambition* and a violent temper master him"; *Commentaries*, Book I, 93; italics in the original.

[39] Sforza to Ottone, Milan, 1 August 1458, cart. 47: Ottone was instructed to make every effort to secure Castiglioni's election; this letter was to be shown to Castiglioni. Sforza to Castiglioni, Milan, 2 August 1458, *PE*–Roma, cart. 47: the duke informed him that he had instructed Ottone to work diligently for Castiglioni's election but urged him to give his vote to Capranica at the appropriate time or run the risk of losing the duke's favor.

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above the heading in favor of an unnamed candidate. Once Ottone was certain of this candidate being elected, he was to insert the name in the appropriate blank space.[40]

The wisdom of the duke's remarkable inventiveness in his efforts to overcome time constraints by taking account of possible eventualities was soon borne out by unexpected developments. Capranica died suddenly on the 14th of August, two days before the conclave began.[41] Castiglioni's cooperation became questionable as he perceived Sforza's true wishes, despite the precautions taken to disguise them. He feigned acquiescence but could not refrain from complaining bitterly that, whereas in Rome he was considered the duke's man, in Milan he was not.[42]

Immediately after Capranica's death, Ottone became convinced that the Cardinal of Siena, Enea Silvio Piccolomini (Pius II), was most likely to succeed, although he was still not without hope for Colonna. In the end, Ottone decided to support Piccolomini, knowing that his candidacy was also backed by Milan's ally, King Ferrante of Naples.[43] I am virtually certain that, on the basis of this last-minute assessment, Ottone filled the blank space in the ducal letter with Piccolomini's name. Unfortunately, this letter has not been found; it was probably destroyed after it had served its purpose.

Even though the powerful support of Milan and Naples played an important role in the outcome, it would likely have failed had Piccolomini not been successful in rallying the Italian cardinals to his side when it became apparent that the Cardinal of Rouen, Guillaume d'Estouteville, was about to win. Ironically, Castiglioni's resentment and ambition contributed to this success. Having bound himself to manage Guillaume's election, he subsequently revealed the secret plan to the Italian cardinals in an effort to gain their support for his own candidacy. His strategy backfired when both sides discovered his treachery and the purpose of his revelations.[44] Without hope for his candidacy, he let himself be persuaded

[40] Sforza to Ottone, Milan, 2 August 1458, Biblioteca Ambrosiana, Cod. Z219 Sup., no. 9464 (orig. almost entirely ciphered, accompanied by the minute in clear, no. 9465). The complexity and nuances of these instructions, which are difficult to convey in a short summary, can be appreciated by reading the full text given here in the Appendix, no. IV. See also two double "S" letters by Sforza to Ottone, same date (*PE*–Roma, cart. 47), again ordering the ambassador to work for Capranica's election and announcing that he was sending letters of credence for him addressed to all the cardinals in order to facilitate his efforts.

[41] Ottone to Sforza, Rome, 14 August 1458, in Pastor, *Storia dei Papi*, 1: 873–74. Two days earlier, Capranica had been pleased to receive Sforza's message of support and still hoped to be elected despite his illness. Ottone had already gained the cooperation of Cardinals Colonna and Pietro Barbo for Capranica's election (Ottone to Sforza, Rome, 12 August 1458, *PE*–Roma, cart. 47).

[42] On receiving Sforza's letters and the message delivered by Ottone, Castiglioni became visibly agitated and indignant at the suggestion that he desired the papal tiara and needed support to obtain it. He then recounted his grievances against the duke, resenting particularly being suspected of passing Milanese secrets to the Cardinal of Rouen, all of which explains at least in part his behavior during the conclave (Ottone to Sforza, Rome, 12 August 1458, Biblioteca Ambrosiana, Cod. Z219 Sup., no. 9372).

[43] Ottone to Sforza, Rome, 14 August 1458, and Galeotto Agnesi to Sforza, Rome, 15 August, in Pastor, *Storia dei Papi*, 2: 679–80 and *Ungedruckte Akten*, 1: 88–90, respectively.

[44] The only detailed account from an eyewitness of this conclave, unique in the annals of papal elections, was written by Pius himself in his *Commentaries*, Book I: 94–103. Throughout this work, Pius hardly mentions Castiglioni without erupting into disparaging comments about him. See especially Book I: 72–73, 88, and Book IV: 308. Pius's account is confirmed for the most part by the information collected by Ottone from other participants (Ottone to Sforza, Rome, 20 and 21 August 1458, in Pastor, *Ungedruckte Akten*, 1: 91–95 and *PE*–Roma, cart. 47, respectively). The following dispatches to Sforza

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by Piccolomini's impassioned appeal to his Italian heritage, and he even echoed these sentiments in announcing the result to Sforza by claiming that he alone had saved the church and Italy from the French.[45]

The unusually detailed documentation about the conclave of 1458 demonstrates the problems faced by influential patrons in recommending candidates without offending other clients. The care taken by Sforza to achieve this objective was not sufficient to prevent the resentment of the ambitious Cardinal of Pavia. Ottone deluded himself in believing that his strategy of extending support to various candidates, on condition that they back the duke's first choice once they had lost hope of being elected, had been carried out with such tact and secrecy that none of them had been offended.[46] Apparently, Ottone was more successful with Piccolomini himself. Although Piccolomini knew that he had not been the duke's first choice, he probably would have been surprised to learn that his name had never been mentioned in Sforza's instructions, and that the Milanese ambassador promoted the candidacy without his master's knowledge. The new pope had no reason to doubt Milan's backing after Capranica's death because Pius enjoyed strong ties of friendship with the duke, and a few years earlier he had labored without success to secure recognition of Sforza's ducal title from Emperor Frederick III, whom he had served as secretary.[47] In fact, it is likely that Sforza would have listed him as one of his favorite candidates, perhaps ahead of Colonna, had he foreseen that the cardinals would seriously consider the election of a colleague who had received the red hat barely twenty months earlier.

In any case, neither in Pius's frank account of his election nor in his expressions of gratitude to Sforza and Ottone did he give a hint of any knowledge he might have had about the last-minute insertion of his name into the specially coded letter.[48] The duke, for his part, hastened to send a message of congratulations to the new pope, in which he recalled their long friendship and boldly stated that he

give somewhat different accounts of the role played by various cardinals: Galeotto Agnesi, Rome, 19 August 1458, *PE*–Roma, cart. 47, and Antonio da Pistoia, Rome, 21 August, in Pastor, *Storia dei Papi*, 2: 680–81.

[45] Alluding to the struggle within the conclave, Castiglioni wrote to Sforza (Rome, 19 August 1458, *PE*–Roma, cart. 47), that he had supported Piccolomini's election for the good of the church and Milan, "lassando alcune passione et anche tale differentie per riguardo de la gloria et exaltatione italica." Later (Rome, 13 September 1458, cart. 47), he urged the duke not to lend credence to false reports about his role in the conclave, which he described as follows: "Nel vero, Signore, se l'opera nostra li fusse manchata et fusse seguita a quello [Cardinal of Rouen] che molti speravano, forsi le cosse seriano conducte in forma che e la chiesa di Dio et tuta Italia ne havrebbe sentito extrema iactura."

[46] In addition to Castiglioni, Ottone had given similar assurances to Cardinals Pietro Barbo and Bessarion, both of whom had requested Sforza's support (Ottone to Sforza, Rome, 20 August 1458, cited in n. 44).

[47] While posing as a champion of imperial authority, Piccolomini appears to have been in collusion with Sforza and his ambassadors (1451–54) in an effort to facilitate the negotiations for the imperial recognition; Fabio Cusin, "Le aspirazioni straniere sul Ducato di Milano e l'investitura imperiale (1450–1454)," *Archivio storico lombardo*, new ser., 1 (1936): 335–54.

[48] In his *Commentaries*, Book I: 106, Pius simply wrote that "Francesco Sforza, though he had expected a different pope, was nevertheless pleased to learn of the election of Aeneas." Of course, supreme egotist that he was, Pius wanted to give the impression that he owed his election only to his ability and to God. In private, he confided to Ottone that he was grateful to him and his master and that he now was in a position to repay them (Ottone to Sforza, Rome, 20 August 1458, cited in n. 44).

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had desired his election more than that of any living person.[49] This self-serving statement conceivably could have deceived even such an experienced diplomat as Piccolomini, especially because he knew that his anti-French posture was consonant with the objectives of Milanese foreign policy. From Sforza's point of view, Pius's unexpected election was truly providential, coming at a time when the French were poised to invade the Kingdom of Naples from their newly acquired base at Genoa in an effort to dethrone Ferrante and install René of Anjou in his place. Pius became Sforza's most cooperative and trusted ally in the defeat of French plans for aggression in the Italian peninsula.[50]

The duke was no less zealous in his use (or misuse) of patronage to safeguard the physical well-being of himself and his family. In an age characterized by intense competition for the services of the most talented in all professions, it is not surprising that the recruitment and retention of skilled physicians should be of paramount concern to persons of rank and means. This concern gave rise to another form of deception practiced at the Milanese court: a letter of recommendation nullified by another letter. The victim of this deception was Benedetto Reguardati da Norcia, who, since about 1438, had entered Sforza's service as his personal physician and sometime ambassador. His reputation as an able practitioner generated requests for his services from various ruling families, including the Bentivoglio, the Gonzaga, and the Medici.[51] Cosimo de' Medici considered him the best physician of his time and had such faith in his skill that during his last illness he refused to be attended by local doctors, preferring to await Reguardati's arrival.[52] In essence, he became Milan's good-will ambassador, sent occasionally on medical missions to friendly courts, provided he gave first priority to the needs of the Sforza family.

Consternation swept the Milanese court in 1451 when Reguardati announced that he wished to resume his teaching and medical practice in Florence, which he had left some years earlier to enter Sforza's service, and requested a letter of recommendation. Unwilling to antagonize a man who held the life of his family in his hands, as it were, the duke obliged, not with one letter but two. In the first, addressed to Andrea Boccaccino Alamanni, he wrote that Reguardati wished to teach and practice in Florence and be nearer to his relatives in Norcia. He also

[49] "Misser Otho, non poresti credere quanto piacere et consolatione ne havemo havuto, et così Dio ne adiuti, ch'el [Piccolomini] l'è stato quello che nuy desyderavamo tanto o più che persona che viva"; Sforza to Ottone, Milan, 23 August 1458, *PE*–Roma, cart. 47. This letter was written within an hour after the receipt of Ottone's dispatch of 19 August announcing Pius's election.

[50] Pius's anti-French foreign policy and his cooperation with Sforza in this area are stressed throughout his *Commentaries*. See also Ilardi, "The Italian League," 151–66; and "France and Milan: The Uneasy Alliance, 1452–1466," in *Gli Sforza a Milano e in Lombardia e i loro rapporti con gli Stati italiani ed europei (1450–1535)* (Milan, 1982), 424–37, 443, for additional documentation.

[51] On Reguardati's career, see Gonario Deffenù, *Benedetto Reguardati, medico e diplomatico di Francesco Sforza* (Milan, 1955), 11–38; and Juliana Hill Cotton, "Benedetto Reguardati of Nursia (1398–1469)," *Medical History*, 13 (1969): 175–89.

[52] Cosimo once commented that Reguardati "è reputato da ciaschuno il più singulare homino in medicina che sia stato a nostri tempi"; Cosimo to Sforza, 5 November 1463, Biblioteca Ambrosiana, Cod. Z247 Sup. Reguardati arrived at Careggi a few hours after Cosimo's death, having been detained in Milan by the illness of the duchess (N. Tranchedini to Sforza, Careggi, 29 July 1464, *PE*–Firenze, cart. 271; and 4 August 1464, Bibliothèque Nationale, Paris, *Fonds Italien*, Cod. 1590, fols. 325r–25v). See also Katharine Park, *Doctors and Medicine in Early Renaissance Florence* (Princeton, N.J., 1985), 115.

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revealed that the doctor was justifiably dissatisfied with his salary, which, regrettably, could not be raised because ducal finances were strained to the limit by the necessity of keeping a large standing army. Since the duchess needed the doctor's services, Sforza asked Alamanni to notify Cosimo and others to disregard the enclosed letter of recommendation on Reguardati's behalf and see to it that under no circumstances he be given the teaching post. Precautions were to be taken to keep the letter secret.[53]

The second letter, written twenty-four days later, was addressed to the Signoria of Florence with copies to Cosimo, Alamanni, and Neri di Gino Capponi, all of whom presumably would be involved in deciding on the appointment. It was a standard letter of recommendation praising the merits of the applicant, expressing regrets over his decision to leave Milan, and ending with the usual exhortation embodying the stock phrases of the age: "We beg and urge you . . . that out of regard and respect for us . . . you will grant us this request . . . for you could not do us a greater favor . . . considering that we bear him such love and are so obligated to him, there is nothing we would deny him."[54] Except for the survival of the preceding letter to Alamanni, we would not know from the lone fact that Reguardati did not receive the appointment that the recommendation was a sham. In the end, the deception achieved its expected result, but the duke also ordered his officials to find funds at all costs to provide adequate compensation for his physician.[55] Had the intended victim outwitted the deceiver this time?

THESE REVELATIONS MAY CONVEY THE IMPRESSION THAT Francesco Sforza had a singular penchant for deception. It would be wise to resist this conclusion until scholars ascertain whether other patrons followed similar practices. The use of codes and ciphers in military correspondence dates back to antiquity and was extended to diplomatic correspondence in the Middle Ages.[56] Such usage could easily have suggested itself to other leading patrons of the Renaissance as an ideal and practical solution of the problem discussed above. Indeed, the fact that the duke secured the willing and at times enthusiastic cooperation of his correspondents—three popes, various prelates, Cosimo de' Medici and his circle—may indicate that the coding of letters of recommendation was not unusual. I am confident that research in the Milanese correspondence with other states will uncover other willing accomplices. At any rate, in the eyes of contemporaries and modern historians alike, Sforza stands out, not as a particularly deceitful prince but as a moderate, humane, and wise ruler with an unrivaled reputation as an

[53] Milan, 1 June 1451, *PE*–Firenze, cart. 265, published by Deffenù, *Benedetto Reguardati*, 138–39, with the wrong archival designation, "Estero-Roma." This letter was addressed to Alamanni, one of Sforza's clients in Florence, because the Milanese resident, N. Tranchedini, was then on a mission to the papal court.

[54] Milan, 25 June 1451, *RM*, 6, fol. 33, published by Deffenù, *Benedetto Reguardati*, 139–40. My translation.

[55] Deffenù, *Benedetto Reguardati*, 19–20.

[56] See Albert C. Leighton, "Secret Communication among the Greeks and Romans," *Technology and Culture*, 10 (1969): 139–54; and Giorgio Costamagna, *Tachigrafia notarile e scritture segrete medioevali in Italia* (Rome, 1968), 33–55.

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astute *condottiere* and diplomat.[57] On the other hand, since war and diplomacy require at times a certain amount of deception, it is likely that he was exceptionally skilled in its use.

Although the evidence presented here indicates that coded letters were used for purposes ranging from the mundane (gifts of cloth) to the sublime (papal elections), researchers should not be unduly alarmed and assume a frequent use of such letters, except in military and diplomatic correspondence, unless additional investigations reveal otherwise. I have not found coded or otherwise suspect recommendations regarding patronage extended to artists and humanists, and it may be that their incidence was rare. (This is a limited sample, however, and Sforza was not a leading patron of arts and letters.) I still believe and hope that the vast majority of recommendations were genuine in content as well as intent, although many of them were undoubtedly perfunctory.

Nevertheless, until more light is thrown on this subject, it would be wise to approach letters of recommendation with more caution than has been shown in the past. Researchers should note unusual signs, especially those next to signatures, and check all available correspondence about a specific request on behalf of a client or else run the risk of reaching erroneous conclusions about the intentions of a particular patron. This additional searching is not an easy task, given the nature of the system, which required extreme secrecy to ensure its success and safeguard the reputation of patrons. In nearly three decades of archival research, the above cases are the first specific references to the system I have encountered with full explanation of the reasons that made it necessary. In the past, I have concluded that some letters of recommendation were questionable or spurious in purpose on the basis of the instructions sent to ambassadors, but, in the course of researching this topic, I have learned that such evidence is not always definitive. Some instructions cited above merely mention unspecified signs to be attached to letters of recommendation, which indicates that they were communicated orally to departing ambassadors. These signs, of course, still remain secret to us. The evidence presented in this article, therefore, is relatively rare. I should note, however, that I made no attempt to analyze systematically the contents of the huge Roman file in Milan, from which most of the evidence is taken, in order to ascertain the exact incidence of coded letters under the first Sforza duke and whether the practice was continued by his successors.[58]

One should not hastily conclude that the rarity of the evidence is primarily due to the secrecy surrounding the system of deception or to its supposed limited diffusion. Rather, it may be largely the consequence of the lack of systematic scholarly analysis of letters of recommendation, the ubiquitous vehicle by means of which Renaissance patronage was most often sought and extended. Only in the

[57] For a brief summary of contemporary opinion and scholarly assessment of Francesco Sforza's achievements, see Ilardi, "France and Milan," 440–46.

[58] Evidence has just been published by a former director of the State Archives in Milan, however, that Francesco's son, Ludovico il Moro, also used coded letters in 1497 to validate his instructions for the dispensing of gifts. The author did not mention coded letters of recommendation and was not aware of the general problem discussed here. See Carlo Paganini, "Due Branda Castiglioni nel contesto amministrativo sforzesco," *Archivio storico lombardo*, ser. XI, 2 (1985; published December 1986): 131.

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past thirty years have historians and social scientists made patronage and patron-client relations a major focus of their research, which may explain in part the surprising neglect of an immense and readily available source that amply documents the bonds of mutual dependence characterizing the patron-client relationship.[59] I am reasonably confident that a thorough exploration of this source will uncover much new material illustrating the relationship, including other examples of coded correspondence or some other system designed for the same purpose. The results of this study may require a reassessment of the dynamics of Renaissance patronage and provide a more precise basis for comparison with similar patron-client relations in other societies across the centuries. It is hoped that the documents cited in this article may prove to be a modest beginning toward this goal by illustrating two fundamental principles governing letters of recommendation—the patron's use of available means to discriminate among recommended clients and the need to exercise a certain degree of caution in interpreting such letters by bearing in mind the maxim *caveat lector*.

[59] In addition to consulting six sociologists, I have searched through the computerized *Social Science Citation Index*, 1972–May 1987, and its printed volumes, 1966–71. Virtually all twenty-six articles listed treat only contemporary issues regarding letters of recommendation, such as confidentiality, reliability, liability, sexual and other discrimination, etc. See also Shmuel N. Eisenstadt and Louis Roniger, "Patron-Client Relations as a Model of Structuring Social Exchange," *Comparative Studies in Society and History*, 22 (1980): 42–77, for the most recent theoretical statement combined with a survey of the literature.

APPENDIX

I

Francesco Sforza to Pope Nicholas V

Lodi, 18 June 1450

Registri delle Missive, 1, fols. 131v–32. Copy.

Copia litterarum scriptarum summo pontifici Nicolao V°.
Sanctissime Patter, me trovo mediante la gratia de l'altissimo Dio, cui immortales gratias habeo, havere acquistato questo stato de Lombardia et non cum puoca fatiga, como la santità vostra ha inteso; mò che l'ho acquistato me bisogna ponere lo pensiero et intelecto ad mantenerlo, governarlo et ponerlo in reposo et tranquillitate. Et perchè li beneficii sonno molto grandemente importanti al facto mio, siando mia totale despositione che la santità vostra possa de questo Stato et de li miei figlioli et fratelli et de la mia persona et ogni mia facultate desponere non altramente che de qualunque altra cosa che sia al suo comando, movo cum cordiale fiducia, zelo et amore aprire cum la santità vostra el nostro bisogno, perchè, se la

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causa di questi beneficii non passasse cum questo ordine, sequeriano ogni dì scandali et inconvenienti in questo mio Stato, che seria puoco reposo de li mei subditi contra la mente de la santità vostra; unde me sò mosso como già per altre mie ho scripto et supplicato la santità vostra che ella se digne ad questi, che vengono là per impetrare beneficii, non conferirli se non portaranno mia lettera. Et perchè li importuni sonno assai, et siando novo in questo Stato, como sò, siando pregato che scriva a la santità vostra per beneficii, non porrò fare che non scriva; per quelli, i quali serà mio desiderio che cum effectu siano compiaciuti da la santità vostra, portaranno littere con li intersegni, como sta questa presente, et me sforzarò che questi tali proponerò saranno persone idonee et digne. Sichè dignese la santità vostra ad quelli che portaranno tale intersigno, conferire lo beneficio per lo quale se scriverà, et non ad altre persone. Et questo non fazo, beatissimo padre, perchè forse volesse torre pagamento de questi tali beneficii, che non fo mia natura nè costume impazarne, como faceva la bona memoria del illustrissimo signor duca passato, ma solo el fazo per ponere questo mio Stato in quiete et reposo, et non per alcuna altra casone. Et però iterato prego et supplico la prefata santità vostra se degni de farne questa gratia honesta, rescrivendome per suo breve como ella sia degnata acceptare questa mia littera et farmi questa gratia, la quale reputarò singularissima. Et se degni la santità vostra fare tenere questa mia littera cum signali talmente secreta quod ad aures Lombardorum non veniat, perchè seria casone de torre molestia a la santità vostra ed ad nui, a la quale me raccomando. Datum Laude, die XVIII junii 1450.

+Francischus Sfortia Vicecomes manu propria subscripsit.

Excellentissime sanctitatis vestre divotissimus filius et servitor Francischus Sfortia dux Mediolani.

Johannes

II

Giacomo Calcaterra to Francesco Sforza

Rome, 31 August 1455

Potenze Estere–Roma, cart. 42. Orig.

[Omitted]. Ill.mo S., accade molto spesso che per parte de Vostra Ex.tia me son presentate littere, nelle qual dispone et ordina Vostra S. habia intercedere per diverse et varie persone in cose però che non perteneno a facti de Vostra Ex.tia, cossì apresso la S.tà del Papa quanto apresso a li R.mi S. Cardinali; nella qual cosa saperebe voluntera che modo habia a tenere et in che forma rezerme et qualle son quelle littere provenneno de pura et mera dispositione de Vostra S. Et in questo harebe a caro me fussi dato qualche contrasegno como più fiate ho richiesto. Et questo scrivo però sò che spesse volte accade che tale littere se fanno cossì per importunità de richidenti, cossì anchora che bon mercato se fa de littere de

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recomendatione et non di mancho quilli, li quali le portono in so favore, quando son qua da me, son molto importuni cossì apressa mi quanto apressa al Papa et cardinali. Il perchè, intendando come ho dicto de sopra, saperò molto ben che forma dare a tute le facende et in che modo gubernarme secundo il volere de Vostra Ex.tia. [Omitted].

III

Francesco Sforza to Giacomo Calcaterra

Milan, 16 September 1455

Potenze Estere–Roma, cart. 42. Orig.

Dux Mediolani etc.

Dilecte noster, perchè accade continuo scrivervi per multe et varie facende et non havendo altro ordene da nuy, nè altro signale de intelligentia, non poresti sapere quale fossero quelle cose in le quale havessevo cum maiore instantia et efficacia operarvi poresti a le fiate errare, dicimo che in li fatti nostri propry fariti secundo le littere haveriti; in questi non accade signale. In le facende de li altri, quando nostra intencione sarà ve rescaldati et faciati con effecto a la expeditione et che ne saranno ad cuore, sottoscriverimo le littere de nostra propria mano et in fine farimo tri SSS, como sta qui de sotto. Non siando le littere sottoscripte, farite quello poreti cum honestate, et siando le littere sottoscripte et siano doi SS, fariti più caldamente che per quelle non saranno sottoscripte; ma quelle haveranno tri SSS, saranno ex intimo cordis et così operariti. Datum Mediolani XVI septembris 1455.

Francischus Sfortia Vicecomes manu propria SSS

Cichus

IV

Francesco Sforza to Ottone del Carretto

Milan, 2 August 1458

Biblioteca Ambrosiana, Cod. Z219 Sup., No. 9465.

Minute

In zifra

Mediolani die IJ augusti 1458. Domino Ottoni de Carreto.
Misser Otto, vuy vedereti quanto ve scrivemo per altre nostre alligate, che

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accadendo la morte del Papa, possiati aiutare el R.mo Mon.re nostro de Fermo che sia assumpto al pontificato. Nuy gli scriviamo una littera de nostra mano propria et gli dicemo largamente l'animo et disposicione nostra verso la Sig.a soa. Tutto el nostro desyderio saria che la Sig.a soa fosse assumptata al pontificato, perchè benchè gli habiamo de l'altri Sig.ri cardinali amici, quali voressemo che havessero bene et ogni honore et exaltacione, tamen et l'animo et el core nostro è tutto volto et desydera ch'el prefato nostro R.mo Mon.re de Fermo fosse quello, perchè siandoli la S. soa, nuy teneressimo che gli fosse la bona memoria del S. Sforza che fò nostro patre. Et perchè volimo che poniati tutto el vostro ingenio et industria ad aiutare questa materia con tutti li vostri sentimenti et non gli lassati mancare cosa veruna, sempre con honestate, perchè consegua questo nostro desyderio perchè excludemo ogni altro, et niuno altro sia chi se voglia ne saria grato et accepto in quella sedia che la Sig.a soa. Al dicto Mon.re, ultra la littera gli scrivamo de nostra mano, porreti ancora monstrare l'altra alligata che scrivamo ad vuy, quale parla del facto suo, porreti ancora monstrare l'altra alligata che drizamo a vuy ad Mon.re de Pavia et monstrare volere operare per luy, et cossì li fareti quello ve parerà bisognare. Ma procedeti in ogni cosa secondo l'ordine ve darà esso Mon.re Firmano, perchè l'intencione et animo nostro è in tutto et per tutto drizato al prefato Mon.re de Fermo. Et cossì pregareti con quella honestà et piacevole via ve parerà esso Mon.re de Pavia, che gli volesse dare la soa voce al tempo como li scrivemo per l'aligate, delle quale ve mandamo le copie. Et non exprimemo el nome in l'una delle littere, in le quale è una croce de sopra in lo capelleto della littera, et in l'altra è lo nome expresso como per dicte copie vedereti, perchè quella senza nome possiati usare secondo el bisogno ve occorerà et in favore de chi ve parerà et siati certi debia cadere la sorte. Sichè non mancati in cosa veruna che ve parà essere possibile de potere fare perchè la cosa cada in esso Mon.re de Fermo. Et quando havesti facto ogni experiencia et opera et che occoresse la sorte non potere cadere in luy, et vuy vedessevo che potesse cadere in Mon.re de Colunna, che è pur nostro parente et quello Sig.re che l'è, et che pur doe volte che è stata facta creacione de novo pontifice ha pur havuto el numero de le voce che non gli n'è mancata non ma una, cossì porria occorere al presente. Occorendo questo caso, vogliate pregare Mon.re de Fermo et Mon.re de Pavia et caduno altro che gli voglia dare la soa voce et fare ogni cosa perchè luy obtenga, perchè nuy ne saressemo molto contenti et de bona voglia et più tosto de luy che de veruno altro sia chi se voglia dapoy dicto Mon.re de Fermo. Se autem la sorte per caso como porria accadere, perchè questi cardinali quando sonno sul facto in conclavi, non obstante le pratiche et promesse se fano fra loro inanti che intrano in conclavi da poy se inganano l'uno l'altro, et ve paresse che non potesse tocare la sorte ad esso de Colunna, volimo che ve adaptati ad aiutare ad fare dare la voce et drizare tutto el nostro favore et aiuto in quello a chi paresse se drizasse la voluntà et animo del prefato nostro Mon.re de Fermo, perchè siamo certi che non sarà non ma persona idonea et digna et italiana et che sarà quello medesmo che è la S. soa verso nuy; certificando esso Mon.re, che papa et cardinale et in qualunche grado che sii la S. soa, porrà sempre disponere de nuy, della persona, del Stato et de quello che havemo como de se stessa. Non volemo dire altro, perchè semo certi ve governareti

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prudentemente, et essendo como sareti sul facto, sapereti ben fare sichè quello che havesse ad essere papa se tenesse da nuy servito et non deservito. Questo non ve possemo insignare, ma vuy che sareti sul facto, siamo certi sapereti meglio fare et exequire in honore, favore et ben nostro che nuy non sapessemo scrivere.

Questa instructione non monstrareti ad persona alcuna, sed sit solum apud vos.

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Review Article

The Flowering of Afro-American History

ROBERT L. HARRIS, JR.

Darlene Clark Hine, ed., prologue by John Hope Franklin, **The State of Afro-American History: Past, Present, and Future** (Baton Rouge: Louisiana State University Press, 1986). Pp. ix, 301. $27.50.

August Meier and Elliott Rudwick, **Black History and the Historical Profession: 1915–1980** (Urbana: University of Illinois Press, 1986). Pp. xi, 380. $34.95.

John Hope Franklin, **George Washington Williams: A Biography** (Chicago: University of Chicago Press, 1985). Pp. ix, 348. $24.95.

Robert H. Abzug and Stephen E. Maizlish, eds., **New Perspectives on Race and Slavery in America: Essays in Honor of Kenneth M. Stampp** (Lexington: University Press of Kentucky, 1986). Pp. ix, 206. $19.00.

IN 1882, GEORGE WASHINGTON WILLIAMS, the subject of John Hope Franklin's spellbinding biography, published his two-volume masterpiece, *A History of the Negro Race in America from 1619 to 1880*.[1] One hundred years later, the Teaching Division of the American Historical Association approved an ad hoc committee, chaired by Darlene Clark Hine, to organize a "state-of-the-art" conference on the study and teaching of Afro-American history. According to conference participant Bettye J. Gardner, "It [was] particularly significant that the 'state of the art' conference . . . should be sponsored by the American Historical Association, for it was this organization's Committee of Seven which was given the charge in 1899 of assessing the 'teaching of history and allied subjects in European as well as American schools.' The basic concern of the committee was the establishment of history as a school subject."[2] But Afro-American history, whether as a subject in its own right or as a part of American history, did not achieve recognition as an appropriate school subject.

Much has changed in the more than one hundred years since the publication of Williams's brilliant study. The Conference on the Study and Teaching of Afro-American History held at Purdue University, October 6–8, 1983, represented the flowering of Afro-American history. With sponsorship from the AHA, the Lilly Endowment, the National Endowment for the Humanities, and Purdue

[1] George Washington Williams, *A History of the Negro Race in America from 1619–1880*, 2 vols. (New York, 1882).

[2] Bettye J. Gardner, "The Teaching of Afro-American History in School and Colleges," in Darlene Clark Hine, ed., *The State of Afro-American History: Past, Present, and Future* (Baton Rouge, La., 1986), 171.

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University, this conference and the subsequent publication of its papers marked the arrival of Afro-American scholarship from the periphery to the core of historical study in the United States. The transition from marginal to mainstream status and the intellectual socialization of those scholars who studied the Afro-American past are the central concerns of Meier and Rudwick's insightful treatise, *Black History and the Historical Profession*.[3]

In a prologue to *The State of Afro-American History*, John Hope Franklin describes four generations of scholarship.[4] The first generation spanned the years from 1882 to 1915, from the appearance of Williams's work to the publication of Booker T. Washington's *Story of the Negro*.[5] This first generation probed the adjustment of Afro-Americans to American life. The second generation, from about 1915 to 1935, was dominated by the Carter G. Woodson school of Afro-American historiography. Woodson and his followers called attention to Afro-American contributions to the development of the United States. The third generation, from roughly 1935 to the 1950s, explored the role of Afro-Americans in the nation's history with less emphasis on achievement and more interest in race relations. This generation attracted an increasing number of white scholars. Franklin identifies the fourth and current generation as the largest and best trained. It has developed a lively and vibrant field of study, broadened the range of enquiry, and entered an arena in which much U.S. history can be reevaluated and rewritten.

Meier and Rudwick's periodization of scholarly generations fundamentally differs from Franklin's. Meier and Rudwick openly acknowledge that "any periodization is of necessity somewhat arbitrary." Even more arbitrary, however, is their starting point in 1915 and their criteria for the sources on which they base their study. Meier and Rudwick specify three major forms of documentation for their book: monographs and articles that constitute the body of Afro-American historiography, interviews with more than 175 historians who have published in the field, and archives and manuscripts. The difficulty in seeking to survey an entire field of scholarship, even one that has remained outside the mainstream for so long, lies in deciding what to include and what to omit.

Meier and Rudwick tend to include only those scholars who have earned doctorates in history and who have published what in their estimation are "significant" articles and monographs. Although an interpretive study of Afro-American historiography requires a process of selection and discernment, the authors appear biased toward the academy as a qualification for membership in the historical profession. Individuals outside colleges and universities are generally excluded from consideration or given fleeting reference. This orthodox and rather narrow method of enquiry perhaps explains why the authors began their work with 1915 rather than with George Washington Williams.

[3] August Meier and Elliott Rudwick, *Black History and the Historical Profession: 1915–1980* (Urbana, Ill., 1986).

[4] John Hope Franklin, "On the Evolution of Scholarship in Afro-American History," in Hine, *The State of Afro-American History*, 13–18.

[5] Booker T. Washington, *The Story of the Negro: The Rise of a Race from Slavery*, 2 vols. (New York, 1909).

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Williams, who wrote the first systematic study of the Afro-American past, enjoyed a varied career as soldier, pastor, editor, columnist, lawyer, legislator, and historian. Born on 16 October 1849, in Bedford Springs, Pennsylvania, he joined the Union Army in 1864 by falsifying his age and assuming a different name. He remained in the military for four years until discharged with a disability, a gunshot wound through the left lung, which he received "not in line of duty." Williams's life was replete with less than salubrious incidents, which the Franklin biography duly notes in giving us an intimate portrait of the subject, warts and all. Franklin does not attempt to explain away Williams's foibles as he takes the full measure of the man. By 1874, Williams had graduated from Newton Theological Institution near Boston, where, according to Franklin, he "had transformed himself in a short span of four years from an uninformed, raw youth to a well-educated man with a felicitous writing style."[6]

That training served Williams well, together with his experience as pastor of Boston's Twelfth Baptist Church, as publisher and editor of *The Commoner* in Washington, D.C., as secretary in the auditor's office of the Cincinnati Southern Railway, and in 1879 as the first black man elected to the Ohio state legislature. While in the legislature, where he completed only one two-year term, he began his study of the Afro-American past. Franklin suggests that Williams was probably inspired by the celebrations surrounding America's centennial and by the positive responses to his talk on Afro-American contributions during the nation's first hundred years of independence. Though not formally trained, Williams used methods of research similar to professional historians of his era. He solicited through newspaper advertisements the minutes and records of black church organizations and information on black schools, economic progress, social conditions, and migration from the South. He interviewed black veterans of the Civil War and their white officers. Williams relied on newspapers as a historical source much earlier than did scientifically trained historians. He sought and received the advice of several historians, among them the doyen of U.S. historians, George Bancroft.

Franklin regards Williams as essentially a revisionist who sought to describe the role of Afro-Americans in the nation's history and development, a story that had been neglected. Although Williams valued objectivity and the scientific approach to recovering the past, he had to admit that, in recounting the story of his people, "many pages . . . have been blistered with my tears." Franklin hails Williams's work as a "major achievement." But, just as he does not shy away from telling about the shortcomings of the man, neither does he ignore the flaws in his work. Franklin tells us that Williams's book was hastily written, that much of its documentation was reproduced without interpretation, that Williams misunderstood the Reconstruction governments and misinterpreted them as Negro-dominated, and that he accepted the "hidden hand of God" analysis of historical events so prevalent during the nineteenth century. Despite its weaknesses, *The History of the Negro Race in*

[6] John Hope Franklin, *George Washington Williams: A Biography* (Chicago, 1985), 11.

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America was a remarkable accomplishment for its time, and Williams certainly merits consideration as a progenitor of Afro-American historiography.

Meier and Rudwick, however, begin their study in the twentieth century and demarcate five periods in the development of Afro-American historiography: 1915–1930, 1930–1945, 1945–1960, 1954–1968, and 1967–1980. In choosing these five periods, they are interested foremost in the relations between the evolution of Afro-American historiography and changes within the society in general and the historical profession in particular. Their major goal is to determine the motivation for scholars to study the Afro-American past, the intellectual perspectives that shaped research, the relationship between scholarly values, ideologies, and writings, the manner in which scholars' perspectives changed over time, the way that intellectual interaction influenced historical interpretation, and the role of philanthropies, publishers, history departments, and professional organizations in advancing the field of enquiry. Given the ambitious agenda Meier and Rudwick have for the book, perhaps it should come as no surprise that there is a tension in their analysis between the development of the field and the socialization of those who shaped it. That tension often obscures understanding, especially of the development of the field.

Although Meier and Rudwick could, by their criteria, exclude George Washington Williams from their schema, they have a more difficult time with W. E. B. Du Bois. The first Afro-American to earn a doctorate in history, Du Bois had his dissertation, *The Suppression of the African Slave Trade to the United States of America, 1638–1870*, published in 1896 as the first volume in the Harvard Historical Studies series.[7] Du Bois was also the first black scholar to participate in the AHA meetings of 1891 and 1909 and to publish an article in the *AHR*.[8] Some seventy years elapsed before the publication of another article by a black scholar, when John Hope Franklin's presidential address appeared in 1979.[9] Despite the impressive credentials that Du Bois possessed, Meier and Rudwick claim that he was "the innovator and prophet . . . [who] stood outside the mainstream of scholarship in the field of Negro History, dominated as it was by Woodson and his associates until nearly the middle of the century."[10] Granted that Woodson exerted a major influence on the growth and development of Afro-American historiography, it is hardly fair to argue that Du Bois was outside the mainstream of scholarship in the field. In addition to his first book, the other works that Du Bois published by 1915, *Philadelphia Negro*, *Souls of Black Folk*, *John Brown*, and *The Negro*, set the contours for much future study of the Afro-American past.[11] From 1897 to 1910, Du Bois also edited fourteen Atlanta University Studies, which examined the socioeconomic status of Afro-Americans against a historical backdrop. Sponsored in part

[7] W. E. B. Du Bois, *The Suppression of the African Slave Trade to the United States of America, 1638–1870* (New York, 1896).

[8] W. E. B. Du Bois, "Reconstruction and Its Benefits," *AHR*, 15 (October 1909): 781–99.

[9] John Hope Franklin, "Mirror for Americans: A Century of Reconstruction History," *AHR*, 85 (February 1980): 1–14.

[10] Meier and Rudwick, *Black History and the Historical Profession*, 13.

[11] W. E. B. Du Bois, *The Philadelphia Negro: A Social Study* (Philadelphia, 1899); *The Souls of Black Folk: Essays and Sketches* (Chicago, 1903); *John Brown* (Philadelphia, 1909); and *The Negro* (New York, 1915).

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by the Slater Fund, the Atlanta University Studies were among the first efforts to measure and record the progress of Afro-Americans.[12] Topics such as the family, church, education, business, employment, crime, and self-help were given careful attention. These reports have been a starting point for many scholars in their studies of the black experience.

CARTER G. WOODSON, THE SECOND BLACK MAN AFTER DU BOIS to earn a doctorate in history from Harvard (1912), played a major role in advancing the field of Afro-American history. Through the Association for the Study of Negro Life and History (ASNLH), which he helped to establish in 1915, Woodson constructed a platform different from Du Bois's to promote Afro-American historiography. Although they functioned in different arenas, both men contributed to the legitimacy of Afro-American history as a field of scholarly enquiry. Woodson used a variety of means to advance the Negro History movement: he founded the *Journal of Negro History* in 1916, organized meetings of ASNLH, initiated observance of Negro History Week in 1926, established Associated Publishers, and in 1937 launched the *Negro History Bulletin* for a popular audience. He attracted young black scholars and sponsored their research through funding secured from foundations.

Woodson sought to promote race progress through the study of history. He believed that the widespread dissemination of scientific truth about black people in Africa and in America would build the self-esteem of Afro-Americans and would vanquish the racial prejudice of white Americans. Woodson convinced philanthropic foundations to support his research program only with the help of several white scholars, such as J. Franklin Jameson, editor of the *AHR* and head of the Department of Historical Research at the Carnegie Institution in Washington, D.C. White scholars served on the ASNLH executive council and participated in its annual meetings. Until mainstream journals and organizations began to recognize the significance of Afro-American history in the 1950s, the ASNLH, *Journal of Negro History*, and Associated Publishers were the principal outlets for scholarship in the field.

Meier and Rudwick note that, of the fourteen Afro-Americans awarded doctorates in history or the history of education prior to 1940, eight were associated with Woodson. To be involved with Afro-American history, according to the authors, was to be affiliated with Woodson and the ASNLH. Though few in number, early black historians were extremely productive and laid the foundation for the study of Afro-American history as a recognized field of scholarship. Many had received fellowships from the General Education Board and the Rosenwald Fund for their advanced degrees. During the Great Depression and New Deal, however, the focus of philanthropic support shifted to policy-oriented research and to improving black colleges as centers of research on Afro-Americans. Woodson, who had previously sustained the ASNLH through grants from the Carnegie Corporation and the Laura Spelman Rockefeller

[12] *Atlanta University Publications* (New York, 1968 reprint of proceedings and papers, 1896–1913).

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Memorial, was forced to rely more on small contributions raised from a large number of Afro-Americans. The proportion of white scholars who held seats on the ASNLH executive council dropped as Woodson looked increasingly to black communities to maintain the organization. Woodson's scholarly productivity declined once he was forced to spend considerable time on fund raising.

The growing number of historians, black and white, who stood outside the Woodson circle, according to Meier and Rudwick, undermined the preeminence of ASNLH. After Woodson's death in 1950, the observance of Negro History Week flagged and attendance at ASNLH meetings declined. Although the civil rights movement rejuvenated interest in Afro-American history, Meier and Rudwick contend that ASNLH slowly came to serve a constituency of teachers rather than productive scholars. By 1972, when the organization changed its name to the Association for the Study of Afro-American Life and History (ASALH), Meier and Rudwick charge, few white scholars attended its meetings. As the authors themselves acknowledge, several changes occurred within American society to make Afro-American history more attractive and to further the careers of historians, black and white, who worked in the field. Rather than being pushed out, many white historians, followed by their black colleagues, abandoned ASALH for the greener pastures of mainstream historical associations and journals.

Publication of Gunnar Myrdal's *An American Dilemma* in 1944 and John Hope Franklin's *From Slavery to Freedom* in 1947 triggered interest in Afro-American history, especially among white historians.[13] The social crisis of the depression and reaction to Nazi propaganda about racial superiority generated equalitarian sentiment. Developments in anthropology and psychology, moreover, undermined the doctrine and practice of racial inequality. Jewish intellectuals and sympathetic white Southerners entered the academy in larger numbers during the years after World War II. White scholars became more sensitive to the significance of Afro-American history for understanding the American experience, while black scholars sought to make Afro-American history an integral part of American history. A marked change took place from the Woodson emphasis on racial pride and unity to the role of Afro-Americans in shaping American society. Historians began to explore slave resistance and black involvement in the abolitionist movement.

By the 1950s and 1960s, Meier and Rudwick contend, the most influential books on Afro-American history were being published by white scholars. The number of black doctorates in history declined during this period, and many of the Afro-Americans who did earn doctorates did not write their dissertations on black history. Ironically, the academy embraced Afro-American history at a time when fewer black scholars were entering the field. One white scholar who did much to quicken interest in Afro-American history was Kenneth M. Stampp. His book, *The Peculiar Institution: Slavery in the Antebellum South*,[14] was, in the words of Robert H.

[13] Gunnar Myrdal, *An American Dilemma: The Negro Problem and Modern Democracy*, 2 vols. (New York, 1944); and John Hope Franklin, *From Slavery to Freedom: A History of American Negroes* (New York, 1947).
[14] Kenneth M. Stampp, *The Peculiar Institution: Slavery in the Antebellum South* (New York, 1956).

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Abzug, a "landmark in the rewriting of Afro-American and race relations history."[15] The essays published in Abzug and Maizlish's *New Perspectives on Race and Slavery in America* by many of Stampp's former students attest to his solid imprint on the field.

The Peculiar Institution opened new interpretations of the Afro-American past. Stampp assumed a unity among Americans that transcended the differences dividing them, writes Abzug. After the publication of *The Peculiar Institution*, white scholars in particular began to examine black history in terms of individual experiences. Even Stanley M. Elkins, whose book *Slavery: A Problem in American Institutional and Intellectual Life*, which appeared in 1959, adopted an environmental rather than a biogenetic argument to explain the existence of Sambo as a childlike, lazy, dependent slave.[16] Stampp and his students have looked at the effects of slavery and racism on both whites and blacks. As Joel Williamson observes in "The Soul Is Fled," "If one created Sambo, one had to create Sambo's keepers . . . In constituting a black world, southerners were constrained to reconstitute the white world as its complement."[17] The manner in which white actions were shaped by the presence or anticipation of black actions is a theme that runs throughout these essays.

William W. Freehling, for example, offers a superb analysis of the Denmark Vesey conspiracy (1822), in Charleston, South Carolina. He informs us that white Charlestonians realized that they could suppress Vesey's primary test of slave loyalty by sending in troops to put down physical resistance, a public act, but that they could not as readily confront his secondary test of slave fidelity in perhaps poisoning a slaveholder's drinking water, a private act. The democrat as despot needed the "Sambo" myth in order to live contentedly and assuage fears of rebellion, whether real or imagined.[18] Similarly, Stephen E. Maizlish in his essay on northern Democratic newspapers explains that race was not the major issue in the 1850s that it became after emancipation, once Northerners anticipated massive black migration from the South.[19]

After emancipation, Leon F. Litwack reminds us, the former slaves had to make choices about their lives of a kind that did not exist during slavery. Litwack brilliantly describes the dialectical tension between white landowners' assumptions and former slaves' expectations. The landowners, however, possessed the upper hand owing to their control of land, credit, the law, courts, and the police. Those few Afro-Americans who achieved a measure of success against these odds were kept in their place by the violence of Southerners. A successful black man was

[15] From the introduction by Abzug in Robert H. Abzug and Stephen E. Maizlish, eds., *New Perspectives on Race and Slavery in America: Essays in Honor of Kenneth M. Stampp* (Lexington, Ky., 1986), 1.

[16] Stanley M. Elkins, *Slavery: A Problem in American Institutional and Intellectual Life* (Chicago, 1959).

[17] Joel Williamson, "The Soul Is Fled," in Abzug and Maizlish, *New Perspectives*, 186.

[18] William W. Freehling, "Denmark Vesey's Peculiar Reality," in Abzug and Maizlish, *New Perspectives*, 25–47.

[19] Stephen E. Maizlish, "Race and Politics in the Northern Democracy, 1854–1860," in Abzug and Maizlish, *New Perspectives*, 79–90.

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considered threatening to social domination and control of the South by whites.[20] James Oakes tells how the large landowners consolidated their political power after emancipation by counting the black population in determining legislative districts, giving blackbelt regions more representation than they enjoyed during slavery. Although black voters were later disenfranchised, the state legislative districts did not reflect their elimination from the polls.[21]

In a useful and informative essay,"'Firm Flexibility': Perspectives on Desegregation in South Carolina," John G. Sproat suggests that the legacy of paternalism and an aristocratic tradition helped South Carolina avert the massive resistance and disruption experienced by several other Southern states during desegregation.[22] He points to the quiet integration of Clemson University in 1963 and the determination of state law enforcement agencies to curb violence. A sense of deference to the upper class among whites made it possible, in Sproat's estimation, for white leaders to contain the white supremacists. The conservatism of black leaders helped avoid the more dramatic confrontations that flared in other parts of the South. Although Sproat acknowledges that disruption existed, such as the clash in 1968 between black students and state police at South Carolina State College in Orangeburg that left three students dead and twenty-eight wounded, he regards that incident as more the exception than the rule. He fails to mention Rock Hill, South Carolina, where the Freedom Riders in 1961 suffered vicious attacks. The lunch counters in South Carolina were hardly scenes of peaceful desegregation. Sproat offers important insights into the demographic profile and political temper of South Carolina during the 1960s but overstates the comparative ease with which the state accepted desegregation.

According to Meier and Rudwick, the civil rights movement did not have as much influence on the historians of its era as World War II and the Korean War had on an earlier generation, especially one of white scholars. Military experience in the desegregated army units of the Korean War affected many future historians. Absorption in black music and interaction with black musicians sparked an interest in the Afro-American past for several. Although Meier and Rudwick's guiding theme is the professionalization of Afro-American history, an important subtheme is race relations within the historical community. At times, their compulsion to determine why white scholars chose to study the black experience almost overwhelms their analysis of the evolution of Afro-American historiography.

The authors probe the many intellectual and societal developments that prompted white as well as black scholars to pursue Afro-American history. While the sit-ins, marches, and demonstrations of the late 1950s and early 1960s affected the black historians, those protests had little effect, in the opinion of Meier and Rudwick, on the white scholars. Many white historians began their research

[20] Leon F. Litwack,"'Blues Falling Down Like Hail': The Ordeal of Black Freedom," in Abzug and Maizlish, *New Perspectives*, 109–27.

[21] James Oakes, "The Present Becomes the Past: The Planter Class in the Postbellum South," in Abzug and Maizlish, *New Perspectives*, 149–63.

[22] John G. Sproat,"'Firm Flexibility': Perspectives on Desegregation in South Carolina," in Abzug and Maizlish, *New Perspectives*, 164–84.

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without an overriding concern for racial justice. The new social history that was facilitated by the use of computers changed the focus of historical study from elites to whole communities. Increased accessibility to sources that reflected black thought and action, such as census materials, tax records, court documents, and voting patterns, were used with greater ease to examine "history from the bottom up." For example, the Booker T. Washington papers at the Library of Congress and collections of black newspapers on microfilm became available to scholars during the 1950s. Greater respect was given to slave narratives, folklore, and music. Historians looked less at the victimization of Afro-Americans and more at how they coped and survived under oppression. Methodological breakthroughs in European history initiated by the *Annales* school in France and studies of the working class in England altered historical scholarship in the United States. The use of interviews and oral tradition came into its own during the late 1960s.

Meier and Rudwick neglect the impact of African studies on historical methodology. Historians of African societies regularly used oral tradition to illuminate the past. Many sub-Saharan African societies preserved their heritage through oral transmission from one generation to the next. Ethnic bards, often called griots, were revered for their ability to recite their people's epic. Art, music, dance, and folklore were also avenues to understanding the African past. In seeking to identify a distinctive Afro-American culture and the imprint of black people on American culture and society, many scholars probed the African background of the slaves transported to the United States. Peter Wood, for instance, in his study of early South Carolina, revealed how the Africans' familiarity with animal husbandry and rice cultivation affected the colony's economy positively.[23] New developments within African studies, especially in the interdisciplinary approach to uncovering the past, probably had as much influence on new perspectives about the Afro-American experience as improvements in European studies.

IN LARGE MEASURE, MEIER AND RUDWICK BRING a Eurocentric point of view to their study, although they recognize the increased significance of Africa to the growth of Afro-American culture as a notable response to conditions in the United States. Absent, however, from their otherwise comprehensive survey of Afro-American historiography is a discussion of Pan-Africanism. Except for a brief reference to E. David Cronon's book, *Black Moses: The Story of Marcus Garvey and the Universal Negro Improvement Association*,[24] the authors ignore a whole body of scholarship on Garvey published since the 1960s.[25] Marcus Garvey, born in Jamaica, spent only

[23] Peter Wood, *Black Majority: Negroes in Colonial South Carolina from 1670 through the Stono Rebellion* (New York, 1974), 36, 61, 88.

[24] E. David Cronon, *Black Moses: The Story of Marcus Garvey and the Universal Negro Improvement Association* (Madison, Wis., 1955).

[25] Randall K. Burkett, *Garveyism as a Religious Movement: The Institutionalization of a Black Civil Religion* (Methuen, N.J., 1978); John Henrik Clarke, comp., *Marcus Garvey and the Vision of Africa* (New York, 1974); Robert A. Hill, ed., *The Marcus Garvey and Universal Negro Improvement Association Papers* (Berkeley, Calif., 1983); Tony Martin, *Race First: The Ideological and Organizational Struggles of Marcus*

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a dozen years in the United States but created one of the largest mass movements ever to emerge among Afro-Americans. His organization profoundly affected black life and thought. Perhaps, because of their Eurocentric perspective, the authors fail to see a nexus between peoples of African ancestry in the New World. They tend to view Afro-Americans through the filter of Americanism without much attention to the place of Africa in Afro-American thought and action. The recent scholarship on Marcus Garvey suggests that the idea of the unity of African peoples and the desire for an independent Africa played a prominent role in Afro-American life. For many Afro-Americans, an emergent Africa was essential to their own quest for racial equality.

A chart or graph on cohorts of historians discussed by Meier and Rudwick during their different eras would have been helpful. With often broad sweeps, they give the regional, religious, economic, and political background of white scholars who wrote Afro-American history. Quantification of these characteristics to the extent possible would have given us greater knowledge of these historians as a group. The authors also suggest that black scholars who published during the 1970s came from more non-ideological backgrounds than did their white counterparts. The black historians allegedly developed their views more on the basis of personal experience of prejudice and discrimination than on the basis of a broad philosophy of social justice. It would be interesting to know the proportion of black historians interviewed by the authors who were born in the North and who attended predominantly white undergraduate schools. Meier and Rudwick do not give the same type of scrutiny to the socioeconomic status, political orientation, and religious affiliation of black scholars as they do to those of white scholars.

The increased interest in Afro-American history, according to Meier and Rudwick, did not lead to any overarching interpretation. Some black scholars sought to draw a distinction between the new "Black History" and the old "Negro History." The latter supposedly rested on more integrationist assumptions, while the former called American society into question more critically. The authors suggest that scholarship by black historians has progressed from a separatist perspective (which was never prevalent among them) to a more universalistic conceptualization of the past. The major tension now is not between integrationist or separatist approaches but between race or class analyses of the black experience. Although greater attention has been given to slave culture and community, no true paradigm has emerged to guide the study of slavery. The authors discovered little intellectual interaction or collaboration among scholars examining slave life. "The new slavery historiography," they observe, "stood at the convergence of a heightened sensitivity in the profession about race discrimination and the importance of the black experience, the cresting of black nationalist ideology, the rise of quantitative and demographic history, and the vogue of research on the

Garvey and the Universal Negro Improvement Association (Westport, Conn., 1976); Tony Martin, *Literary Garveyism: Garvey, Black Arts, and the Harlem Renaissance* (Dover, Mass., 1983); and Emory J. Tolbert, *The UNIA and Black Los Angeles: Ideology and Community in the American Garvey Movement* (Los Angeles, 1980).

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history of the inarticulate."[26] We may not call the result of the new interpretations of slavery a paradigm, but, for many historians, this different conceptualization provided a pattern of analysis and interpretation.

For Meier and Rudwick, the recent expansion of Afro-American history stands in grave danger of contraction. They worry about the prominence of school teachers and Black Studies professors in the ASALH. They are troubled by attitudes that lead faculties to hire black scholars to teach Afro-American history and that inhibit, in their view, research by white scholars. "The manner in which Afro-American history is still regarded as a teaching and research specialty particularly suitable for blacks," they conclude, "suggests that the field continues to occupy something of a marginal place."[27] This is a somber note on the part of two white scholars who have devoted their professional lives to illuminating the Afro-American past and thereby providing greater insight into American society. This volume marks the culmination of a brilliant career for Elliott Rudwick, who died shortly after its publication, and with it the authors have provided an intriguing panorama of Afro-American historiography during the twentieth century. They have raised issues that should keep students of the Afro-American past occupied for some time.

The State of Afro-American History: Past, Present, and Future was specifically designed to examine the research agenda for deepening an understanding of the black experience. As several of its authors explain in their essays, an awareness of the Afro-American past offers the potential for a more complete rendering of the American saga. Thomas C. Holt, in his deft introduction, "Whither Now and Why?," succinctly states the case for the book and for the field itself. He informs us that "some of the papers are essentially intellectual and historiographical in focus; others, dealing with issues that are necessarily political and institution-oriented, might be considered to be of a different sort entirely. They might seem to be; but they are not, because intellectual and political considerations are pervasively connective and interactive, especially in Afro-American history."[28] This statement clearly acknowledges that the study of Afro-American history, if indeed not all history, does not reside exclusively within the domain of the academy. As Holt reminds us, the "creation of a history-conscious public is crucially linked to the creation of histories."[29] *The State of Afro-American History* spans the major areas of scholarship in the field and its dissemination through textbooks, history courses, museums, and film.

Despite the acceptance of Afro-American history by the historical profession, James D. Anderson notes in "Secondary School History Textbooks and the Treatment of Black History" that, "as recently as 1982, evaluators of textbooks were in agreement that the accounts of Afro-American life and culture were

[26] Meier and Rudwick, *Black History and the Historical Profession*, 271.
[27] Meier and Rudwick, *Black History and the Historical Profession*, 307.
[28] Thomas C. Holt, "Introduction: Whither Now and Why?," in Hine, *The State of Afro-American History*, 1.
[29] Holt, "Introduction," 9.

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superficial, misleading, and inadequate."[30] Historians have explored practically every area of the black experience, but much of the latest research has not found its way into American history textbooks. Nathan I. Huggins, in "Integrating Afro-American History into American History," calls for a new synthesis of American history. He criticizes textbooks that offer separate sections on Afro-Americans rather than including them in the main story.[31]

This volume has much to offer scholars, teachers, administrators, and history buffs. Historians on the frontier of recent developments in the field , such as Leslie H. Owens, Armstead L. Robinson, and Kenneth L. Kusmer, discuss the latest research in slavery, emancipation, and urban studies. The editor of this signal work, Darlene Clark Hine, expertly analyzes the trajectory of black women's history. She remarks that "by the late 1890s there developed a major division of emphasis within the black protest tradition. Black men attacked racial discrimination as it operated in the public corridors of power. Black women, whose center of influence had always existed primarily in the family, in the church, and in their female associations, believed that part of the overall struggle for true racial advancement depended upon the extent to which they obliterated all negative sexual images of themselves."[32] If there is one major weakness in this work, it is a lack of attention to the tradition of black protest in its various dimensions, particularly during the civil rights era. William H. Harris, however, does call for more study of the black working class, black nationalism, and black protest during the civil rights period. He suggests, in "Trends and Needs in Afro-American Historiography," a reconsideration of black radicalism, which too often has been associated with slave revolts or leftist politics. To seek civil rights in the American South during the late 1950s and early 1960s, he notes, was a radical step.[33]

Although Afro-American history has flowered, particularly within the historical profession, Meier and Rudwick are certainly correct about the need for nurturing if it is to continue to blossom. As Carole Merritt points out in "The Power of History: Interpreting Black Material Culture," "access to the black past is hampered not so much by a dearth of evidence as by the tedious task of compiling and analyzing large quantities of data."[34] She refers specifically to the evidence of material culture, the three-dimensional products of the black experience that museums have not widely preserved and that historians have not employed in their research, even when available. Consequently, there are several media within which Afro-American history should be nurtured. The works under review here offer in varying ways useful suggestions for guidance and direction.

[30] James D. Anderson, "Secondary School History Textbooks and the Treatment of Black History," in Hine, *The State of Afro-American History*, 261.

[31] Nathan I. Huggins, "Integrating Afro-American History into American History," in Hine, *The State of Afro-American History*, 166–67.

[32] Darlene Clark Hine, "Lifting the Veil, Shattering the Silence: Black Women's History in Slavery and Freedom," in Hine, *The State of Afro-American History*, 234.

[33] William H. Harris, "Trends and Needs in Afro-American Historiography," in Hine, *The State of Afro-American History*, 152.

[34] Carole Merritt, "The Power of History: Interpreting Black Material Culture," in Hine, *The State of Afro-American History*, 214.

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Review Article

Public History's Past, Present, and Prospects

W. ANDREW ACHENBAUM

Susan Porter Benson, Stephen Brier, and Roy Rosenzweig, eds., **Presenting the Past: Essays on History and the Public** (Philadelphia: Temple University Press, 1986). Pp. xxiv, 424. Cloth $34.95, paper $14.95.

Barbara J. Howe and Emory L. Kemp, eds., **Public History: An Introduction** (Malabar, Fla.: Robert E. Krieger, 1986). Pp. vii, 508. Cloth $39.95, paper $27.50.

Richard E. Neustadt and Ernest R. May, **Thinking in Time: The Uses of History for Decision Makers** (New York: Free Press, 1986). Pp. xxii, 329. $19.95.

David F. Trask and Robert W. Pomeroy III, eds., **The Craft of Public History: An Annotated Select Bibliography** (Westport, Conn.: Greenwood Press, 1983). Pp. xix, 481. $49.95.

DURING THE PAST DECADE, a growing number of "public" and "applied" historians have been redefining the scope and purposes of their professional activities. The books under review document a widespread interest in, and innovative approaches to, serving the needs of nonspecialists. *Public History: An Introduction* offers a balanced (if bland) guide to the field's intellectual and institutional precedents and its current range of objectives and activities. *Presenting the Past* also surveys recent scholarship and professional interaction, but, since many of the essays are based on articles written for the *Radical History Review*, a different set of ideological assumptions and activist positions is manifest. *The Craft of Public History*, prepared under the auspices of the National Council on Public History, is an annotated bibliography that includes 1,700 citations to theoretical works, how-to guides, and other examples of the genre. *Thinking in Time* is the best primer available for teaching nonhistorians how to incorporate insights from the past into the decision-making process. These works underscore the diversity of current practice in the field and the considerable divergence of opinions concerning the relevance of history for an understanding of contemporary societal problems. By what they affirm as well as by what they do not say, the four books show the ways in which public history might be rendered more useful through cross-fertilization with mainstream history.

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"In its simplest meaning," observed Robert Kelley, "public history refers to the employment of historians and the historical method outside of academia."[1] Many public historians expect to work outside educational institutions. The thirty-one contributors to *Public History: An Introduction* illustrate this fact. Only four of the authors hold regular teaching appointments; another four are adjunct professors who occasionally offer public history courses in their field of expertise. By contrast, nearly one-third of the contributors, are federal employees: Ph.D.s who work for the U.S. Army Corps of Engineers, the U.S. Department of Agriculture's Forest Service, the National Park Service, the National Museum of American History, the U.S. Air Force, and the Senate Historical Office. Others have jobs in state agencies, county museums and archives, and city halls. Several serve as public-works administrators at various levels of government. Finally, roughly one-quarter of the authors are employed in the private sector. One of them writes of his work supervising the Coca-Cola Company's business archives; another describes the problems and challenges that public historians face when they establish their own consulting businesses and research groups. The rest engage in historic preservation and industrial archaeology, work as editors or publishers, or develop interpretive media in museums or for libraries and historical organizations.

The preponderance of scholars with nonacademic affiliations who are contributors to *Public History: An Introduction* is not necessarily representative of the subset of professionals who might be considered primarily "public" or "applied" historians. After all, ten of the twenty-two contributors to *Presenting the Past* have full-time teaching positions, four hold joint or adjunct appointments, and four others were completing their dissertations at the time they wrote their articles. Only three writers hold nonuniversity posts; the job status of one other is unclear. The authors of *Thinking in Time* have chairs at Harvard. Seven of the thirteen chapter editors of *The Craft of Public History* have academic appointments (though not always in history), and another holds a joint appointment. The curricula vitae of the other six attest to their ability to cope with exigencies and unlikely opportunities found in the scholarly marketplace. Theodore Karamanski is an associate professor at Loyola University of Chicago and was director of the Mid-American Research Center in 1983. Richard Kesner was founding director of the Archives of Appalachia at East Tennessee State University before becoming manager of Office Systems and Services for the F.W. Faxon Company. David Trask taught at several universities before working as director of the Office of the Historian at the U.S. State Department and then as chief historian of the U.S. Army Center of Military History. Suellen Hoy, a prominent figure in the movement, became assistant director of the North Carolina Division of Archives and History after serving as executive director of the Public Works Historical Society and as editorial assistant at the *Journal of American History*.

[1] Robert Kelley, "Public History: Its Origins, Nature, and Prospects," *Public Historian*, 1 (1978): 16. Kelley is one of the architects of the pioneering Graduate Program in Public Historical Studies at the University of California, Santa Barbara. For his latest views, see Daniel Greer, "Insight," *Public History News*, 7 (1987): 2. See also references at chap. 1 (sect. III), chap. 2 (sect. I), and chap. 11 (sect. I) in Trask and Pomeroy, *The Craft of Public History*.

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It is not unprecedented to find historians pursuing meaningful careers outside the academy. Beginning in the nineteenth century, the Army, Navy, and Department of State paid some historians to collect, edit, and publish military records, treaties, and laws. By the 1930s, a few professionally trained historians worked full-time for these organizations as well as for the National Archives, the Department of Agriculture, and the National Parks Service. And yet not until World War II did the U.S. government employ large numbers of historians.[2] By 1978, about 800 of them worked in more than 125 units for historical research. Subsequent surveys report further increases in government-employed historians, even without counting archivists, historians employed by nonreporting agencies, and other comparable professionals outside category GS–170, "federal historians."[3] State and local archives and museums collectively represent an even greater source of employment. The American Association for State and Local History identified 3,300 such organizations in 1970; the number had quadrupled by 1986.[4]

The proliferation and expansion of public and private bodies that require professionals to write their "official" histories and to manage their archives and artifacts has expanded the job market for historians. As fierce competition continues for the relatively small number of academic positions, historians have found alternative careers in business, applying their talents and training in nontraditional ways. According to Robert W. Pomeroy III, deputy adviser of the Inter-American Development Bank, the analytic, research, writing, and teaching skills of historians suit them well for positions in information management, human resources, corporate planning, and marketing.[5] Indeed, contrary to the assumption of most university professors, many scholars prefer to do research and writing in nonacademic settings. Consider, for instance, Thomas M. Doerflinger, who recently won the American Historical Association's Herbert A. Feis Prize and shared a Bancroft Prize for his profile of Philadelphia's eighteenth-century merchants, *A Vigorous Spirit of Enterprise*. Doerflinger spends much of his time doing research for Paine Webber's investment policy committee and forecasting the future on the basis of trend analysis.[6]

[2] For more on this in Howe and Kemp, *Public History: An Introduction*, see Fredric Miller, "Archives and Historical Manuscripts," 37; Martin Reuss, "Public History in the Federal Government," 294–98; and Ronald W. Johnson, "History in the National Park Service," 375.

[3] Richard G. Hewlett, "The Practice of History in the Federal Government," *Public Historian*, 1 (1978): 34–35. For recent trends, see the 1978, 1981, and 1984 issues of the National Coordinating Committee for the Promotion of History, *Directory and Survey of Historical Offices and Programs in the Federal Government* (Washington, D.C.). To put these statistics into comparative perspective, there were some 6,600 economists in the federal government by 1940, according to estimates by the Civil Service Commission. World War II drew countless more social scientists from all fields into government.

[4] Gerald George, "The American Association for State and Local History," in Howe and Kemp, *Public History: An Introduction*, 259. See also Bruce W. Dearstyne, ed., "Archives and Public History: Issues, Problems, and Prospects," *Public Historian*, 8 (1986): 5–98.

[5] Robert W. Pomeroy III, "Public History: Business Management," in Trask and Pomeroy, *The Craft of Public History*, 59–61. In addition to his work at the Inter-American Development Bank in Washington, D.C., where Pomeroy has served for two decades, he has consulted since the 1970s for a number of programs in public and applied history. Pomeroy is a founding director of the National Council on Public History.

[6] See his *Back to the Future: America's Second Great Restructuring* (New York, 1986); and Doerflinger and Jack Rivlan, *Risk and Reward: Venture Capital and Making of America's Great Industries* (New York, 1987).

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NEED FOR EMPLOYMENT IS NOT THE ONLY FACTOR motivating public historians. Nearly every contributor to *Presenting the Past* emphasizes that getting involved in "public history" is a way to promote "people's history." Consonant with the aims of the lyceum movement, Chautauqua-style adult study groups, and the Socialist party history clubs before World War I, more and more professional historians are choosing to work with, not just for, the public. They produce folklife festivals, collaborate with feminist filmmakers, conduct workshops for people interested in gathering oral histories and pursuing genealogical research, and help neighborhood groups to design projects in living history. Besides enabling senior citizens, homosexuals, women, ethnic and racial minorities, and union members to rediscover their roots, some practitioners have ideological and political aims. Impelled by the politics of protest that flourished in the 1960s and by the intellectual ferment that sustains radical historiography, these historians contribute to a contrapuntal movement in public history that strives "to encourage a progressive, accessible, and frequently oppositional historical vision."[7]

Two essays in *Presenting the Past* illustrate the leftist style of doing public history. Both contributions are by Michael Wallace, editorial coordinator of the *Radical History Review* and a director of the New York City Public History Project, who also teaches at John Jay College. In the first of these contributions, "Visiting the Past," Wallace focuses on the kinds of perspectives that history museums in the United States offer visitors and on the motives that inspired people to create ancestral societies. In a highly readable, amply documented review, he traces the formative years of the Mount Vernon Ladies' Association (founded in the 1850s), the Daughters of the American Revolution (1890), and the Society for the Preservation of New England Antiquities (1910), as well as Henry Ford's Greenfield Village, John D. Rockefeller's Colonial Williamsburg, and initiatives undertaken by the National Park Service during the Great Depression. Wallace's treatment of postwar developments seems fair and accurate. He portrays sites such as Old Sturbridge Village, which opened in 1946, and the Living Historical Farm movèment, which spread in the middle and late 1960s, as ventures concerned with exploring work and family life; he also pointedly explains why slavery was not "discovered" at Williamsburg until the 1970s.

Only in the concluding section does Wallace elaborate his ideological stance. He begins by citing J. H. Plumb's observation that the "acquisition of the past by ruling and possessing classes and the exclusion of the mass of the peasantry and labor class is a widespread phenomenon through recorded history."[8] Wallace then asserts that the establishment of history museums and exclusive societies were ways that America's "dominant classes . . . appropriated the past." Assuming the role of supportive critic, Wallace makes recommendations for changing the situation.

[7] Susan Porter Benson, *et al.*, "Introduction," *Presenting the Past*, xvii. Three activist historians, for instance, formed the Massachusetts History Workshop in 1978. Their aim was to use the participatory democracy model for doing history. Among recent activities, worker historians, university scholars, a leftist foundation, women's groups, and the state AFL-CIO joined forces to commemorate the founding of the Women's Trade Union League.

[8] Plumb originally made the remark in *The Death of the Past* (Boston, 1970), 30; it is quoted in Michael Wallace, "Visiting the Past," in Benson, *et al.*, *Presenting the Past*, 158.

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Museums must become "places that deal with politics as well as culture," able to teach their visitors about the complexities and contrarieties of a past that cannot be disconnected from the present. "Museums should consider it their fundamental mission to assist people to become historically informed makers of history."[9]

Wallace pursues a similar tack in his second contribution, "Reflections on the History of Historic Preservation." Data, not rhetoric, determine the story line. He notes that for antebellum Americans "'progress' took precedence over preservation."[10] He discusses four groups that between 1880 and 1940 protested the casual destruction of the past. Wallace offers fascinating insights into the ways in which various groups of conservationists—the National Trust for Historic Preservation, blacks, yuppies—benefited and subsequently suffered (especially under Reagan) because of larger political forces and shifts in the historical climate. Wallace urges architects, historians, park service professionals, local history amateurs, neighborhood activists, and history enthusiasts to forge a new preservation coalition. Once again, Wallace meshes sound analysis with a passionate vision and demonstrates that public history will increasingly become an arena for the same type of partisan rancor and debates over fundamental goals that currently animate discussions in faculty lounges.

For all their seeming comprehensiveness, the books under review do not fully cover the present range of public history. Focusing almost exclusively on the United States, they ignore parallel developments in Western Europe. For example, historic preservationists on both sides of the Atlantic are seeking the best ways to develop sites and display relics in order to educate and entertain modern-day visitors.[11] Dutch politicians have hired historians to investigate suspicious actions by recent public officials. British historians are creating "applied" history programs modeled on the curriculum that Peter N. Stearns and Joel A. Tarr established at Carnegie Mellon University, which incorporates social science theory and normative considerations in the analysis of twentieth-century trends. Others are doing archival research sponsored by private agencies.[12] Scholars outside America have advanced further than U.S. historians in delineating the structural and cultural factors that have influenced officials in most advanced industrial societies to create a welfare state to address and remedy the needs of the poor, the disenfranchised, and the old. Their use of Marxist and neo-Marxist theory is often more sophisticated than the political economy models currently being employed in this country by historians and social scientists to describe and explain distinctive features of American social welfare and employment policies.[13]

[9] Wallace, "Visiting the Past," 160–61.

[10] Wallace, "Reflections on the History of Historic Preservation," in Benson, *et al.*, *Presenting the Past*, 168.

[11] David Lowenthal, *The Past Is a Foreign Country* (Cambridge, 1986).

[12] Anthony R. Sutcliffe, ed., "The Debut of Public History," *Public Historian*, 6 (1984): 6–97; and Michael Drake, ed., *Applied Historical Studies* (London, 1973). Leslie Hannah's *Inventing Retirement: The Development of Occupational Pensions in Britain* (Cambridge, 1986) was underwritten by England's largest pension concern. For more on the Carnegie Mellon curriculum, see Peter N. Stearns and Joel A. Tarr, "Curriculum in Applied History: Toward the Future," *Public Historian*, 9 (Summer 1987): 111–25.

[13] See, for instance, B. Murray, *The People's Budget, 1909–10* (Oxford, 1980); Peter J. Flora and Arnold J. Heidenheimer, eds., *The Development of the Welfare State in Europe and America* (New Brunswick,

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These books largely overlook the role played by historians who serve on official commissions, engage in litigation support, or work for public policy research centers. The National Commission on the Causes and Prevention of Violence recruited historians in 1968 to work on a task force charged with providing historical and comparative perspectives. Its report, *Violence in America*, was reprinted by three major trade publishers and sold over 300,000 copies.[14] Historians actively participated in the project on Children and the Aging Society sponsored by the Carnegie Corporation in New York.[15] Eight historians at the RAND Corporation are paid to do policy relevant research on domestic priorities and national security issues.[16] Conspicuous by its absence in these accounts—to cite only one case—is any mention of the Twentieth Century Fund, an independent foundation that sponsors policy studies of economic, social, and political institutions and issues and often uses the expertise of historians.[17]

Policy centers, however, rarely go out of their way to hire historians. When the Urban Institute sought to place the aims of the Reagan administration "in the larger context of America's evolving public purposes," policy analysts and professors of education, sociology, public finance, economics, government, and international affairs—but no historians—were asked "to stand as far back from very recent history as the short passage of time would permit and assess the probable significance of this history for the future."[18] For that matter, few employers, public or private, are ready to acknowledge that historical training eminently qualifies individuals for positions far removed from classrooms and archives.

Precisely because of competition for employment with other academically trained professionals, public historians contend that they must apply their craft differently from university-based researchers and teachers. Public historians are "stretched far beyond the bounds of anything they were prepared to do when they made the choice of becoming professional historians," wrote Larry E. Tise, executive director of the Pennsylvania Historical and Museum Commission. "Professionalism itself is challenged by the demands of making deadlines and

N.J., 1981); W. Mommsen, *The Emergence of the Welfare State in Britain and Germany* (London, 1981); Peter Kohler and Hans Zacher, *The Evolution of Social Insurance, 1881–1981* (Munich, 1982); John F. Myles, *Old Age in the Welfare State: The Political Economy of Public Pensions* (Boston, 1984); and Paul Johnson, *Saving and Spending: The Working-Class Economy in Britain, 1870–1939* (Oxford, 1985).

[14] Hugh Davis Graham and Ted Robert Gurr, eds., *Violence in America*, rev. edn. (Beverly Hills, Calif., 1979), 9–13.

[15] John Demos, "History and the Formation of Social Policy toward Children: A Case Study," reprinted in John Demos, *Past, Present, and Personal: The Family and the Life Course in American History* (New York, 1986); Alan Pifer and Lydia Bronte, eds., *Our Aging Society: Paradox and Promise* (New York, 1986).

[16] Steven Schlossman, unpublished paper, Carnegie Mellon University, 24 November 1986.

[17] The Twentieth Century Fund has recently contracted historians to write books on the difficulties besetting municipal pensions, the Social Security system, and federal disability programs. See Robert M. Fogelson, *Pensions: The Hidden Costs of Public Safety* (New York, 1984); W. Andrew Achenbaum, *Social Security: Visions and Revisions* (New York, 1986); and Edward D. Berkowitz, *Disabled Policy: America's Programs for the Handicapped* (New York, 1987). Otis L. Graham, Jr., and James A. Smith are completing their respective manuscripts on the future of American industrial policies and on the rise of think tanks such as Brookings, the Commonwealth Fund, and the Heritage Foundation.

[18] John L. Palmer, ed., *Perspectives on the Reagan Years* (Washington, D.C., 1986), xii.

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forcing the products of research and interpretation to fit into small spaces and alien formats."[19] Yet, paradoxically, public historians who recognize and even accentuate the unique nature of their audiences, styles, and methods cause stubborn doubts about the legitimacy of their enterprise in the minds of historians in educational settings.

Indeed, based on my own experience and on insights gained from reading between the lines of these books, I believe that public historians must grapple every day with questions about how to do history—such as solo versus collaborative ventures, basic versus applied research, and short-term objectives versus long-term goals—that rarely preoccupy more traditional scholars. Most historians are trained to work alone. They expect to write articles and books for which they alone are responsible. In contrast, few public historians engage in independent scholarship. Radical scholars, in fact, emphasize the importance of purposely interacting with the audiences they hope to serve. "Whether a nonhierarchical, democratic, and community-based historical practice can be merged with a theoretical understanding of class, racial, and sexual oppression depends only in part on the energy and vision of those committed to developing a people's history in the United States," assert the editors of *Presenting the Past*. "Grasping the contingent nature of the past can break the tyranny of the present; seeing how historical actors made and remade social life, we can gain a new vision of our own present and future. That is perhaps the most important lesson historians can help people to draw from the past."[20]

Public historians appreciate that they may pay a price for their decision. The greatest prestige and choicest rewards in academe typically go to those who engage in basic, disciplinary-specific research. Activities such as helping neighborhood groups celebrate the efforts of those who once lived in their community and enabling workers to come to grips with the historical significance of struggling to establish a union are laudable in themselves but count for less in decisions about merit raises and tenure than works of scholarship judged to be of high quality by a committee of peers. Historians who work outside the academy invariably face a different set of problems concerning the purposes of their research. "Ultimately, the corporation is the public historian's 'client' and boss simultaneously. Those who labor within its confines or consume its products remain less subjects of these historians than audiences for them. However impartial the historian, the partisan nature of the corporate work environment sets clear boundaries."[21]

Most historians in academe assume that they can set their own timetable for completing an article or submitting a book-length manuscript to a publisher. Reasonably confident that they know what they want to say, they struggle to produce works of scholarship that reflect their best efforts and convey their voice to prospective readers. Public historians rarely enjoy this luxury. Since publishing

[19] Larry E. Tise, "The Practice of Public History in State Government," in Howe and Kemp, *Public History: An Introduction* 334.
[20] Benson, *et al.*, "Introduction," *Presenting the Past*, xxiii–xxiv.
[21] Daniel J. Walkowitz, "Corporate History, or Giving History the Business," in Benson, *et al.*, *Presenting the Past*, 225.

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Social Security: Visions and Revisions, I have been summoned to write guest editorials due on a nameless editor's desk in two hours and requested to draft two to three-page policy "bullets" that are either supposed to praise a proposal under consideration in Congress or condemn ideas advanced by high-ranking public officials who, with little expertise, discuss Social Security taxes or generational equity issues. More often than not, the foundation head or Washington staff member who asked me to drop everything moves on to defusing another "crisis" before having time to introduce my arresting insights into the public record. Meanwhile, I wrestle with my conscience: have I gone beyond analyzing issues to become a self-serving partisan in the advocacy process? Though trained in the production of polished monographs and articles, public historians, it seems, must learn to report in brief and in short order to nonspecialists, people who put no premium on disinterested and detailed scholarship.

Distinctions can be overdrawn, of course. Untenured faculty members face pressures similar to those confronting public historians. Not-so-junior faculty often have to juggle multiple objectives, including publication deadlines, the quest for research money, and—would that the last were first—the satisfaction of other scholars who read their work. Even many of the "stars" of the profession must deal with the same types of risks that bedevil professional colleagues who work in government and the private sector. Alfred D. Chandler edited the Eisenhower papers, directed the Center for the Study of Recent American History, and chaired the Johns Hopkins University history department while doing his own pathbreaking research. C. Vann Woodward did not anticipate (and could not control) the expedient uses made of *The Strange Career of Jim Crow*.[22] Scholars who help prepare legal depositions or who serve as expert witnesses in court cases are often denied access to evidence that might prove useful to the opposing side. Under examination, they may likewise be barred from introducing nuances critical to a trial by peers—witness the debate between Alice Kessler-Harris and Rosalind Rosenberg over their respective efforts to present complex historical analysis in *Equal Employment Opportunity Commission v. Sears, Roebuck and Company*.[23]

[22] C. Vann Woodward, *Thinking Back* (Baton Rouge, La., 1986), 90–95. See also John Hope Franklin's impressions in "The Historians and Public Policy," in Stephen Vaughn, ed., *The Vital Past: Writings on the Uses of History* (Athens, Ga., 1985), 347–59; and Thomas K. McCraw, "The Challenge of Alfred D. Chandler, Jr.: Retrospect and Prospect," *Reviews in American History*, 15 (March 1987): 160–78.

[23] In 1979, the EEOC filed a discrimination suit against Sears, charging sex discrimination in two main categories: commission sales and wage differentials between women and men in managerial and administrative positions. Rosenberg, one of Sears' expert witnesses, claimed that males and females have had different attitudes, goals, and expectations toward work throughout American history. Kessler-Harris countered that the sexual division of labor has been a malleable concept. Despite an ideology of domesticity, women have consistently taken better-paying jobs when afforded the opportunity. Judge John A. Nordberg ruled on February 3, 1986, that the EEOC had not proven its charges. Rosenberg's offer of proof and Kessler-Harris's written testimony are reprinted in *Signs*, 11 (Summer 1986): 751–79. For more on the case, see Ruth Milkman, "Women's History and the Sears Case," *Feminist Studies*, 12 (Summer 1986): 375–400; and Darcy O'Brien, "A Coming of Age," *Savvy*, 7 (October 1986): 73–75, 83. This controversy is not unique. See Lisa Duggan, "History's Gay Ghetto," in Benson, *et al.*, *Presenting the Past*, 289; Philip F. Mooney, "The Practice of History in Corporate America," in Howe and Kemp, *Public History: An Introduction*, 435; and Leland R. Johnson, "Public Historians for the Defendant," *Public Historian*, 5 (1983): 65–76.

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Despite much insistence on the need to experiment with new methods of presentation, nearly every essay in these books was written by a single author who adhered to traditional modes of professional discourse. Most "applied" historians routinely collaborate with specialists from other disciplines and backgrounds. Today, academic historians often work in similar ways. Joint ventures are almost inescapable when editing private papers, preparing historical encyclopedias, or compiling biographical dictionaries. Historians frequently work with economists, sociologists, anthropologists, and political scientists in creating data bases for interdisciplinary research. A few scholars, such as Theodore Hershberg, have embraced new research goals and forged new partnerships through "the purposeful study of history to shed light on issues of contemporary social policy."[24] On close examination, the methodological boundaries between public history and academic history appear to be quite permeable.

THE REAL ISSUE AT STAKE IN THE DEBATE over public history's legitimacy is not, fundamentally, a question of employment status, of *modus operandi*, or of which specialties are relevant. The question is why a professional historian's perspective is important in analyzing public policy or in convincing ordinary people of the importance of the past in their everyday lives. The most straightforward answer—that history is "useful"—calls forth little resonance among the mainstream historians surveyed here and would be qualified by *bona fide* "applied" researchers. "A search for the *practical* use of history invariably ends in disappointment," observes Lester D. Stephens, a historian at the University of Georgia who wrote *Probing the Past: A Guide to the Study and Teaching of History* (1974).[25] Not all the scholars who might appear to be quintessentially public historians subscribe to claims of "relevance." Arthur M. Schlesinger, jr., believes that a deep knowledge of history generates greater insight and sharper diagnostic aptitude in policy making. But Schlesinger's historical research and firsthand experience indicate that George Santayana's aphorism ought to be reversed: "Too often it is those who *can* remember the past who are condemned to repeat it."[26] Daniel M. Fox, a Beveridge Prize-winning historian who divides his time between combing archives and directing the New York State Center for Assessing Health Services, is even more skeptical: "I am a critic of historicism—of the belief that there is a purpose in history, that events in the past unfold toward the present, and that history teaches."[27]

[24] Theodore Hershberg, ed., *Philadelphia: Work, Space, Family, and Group Experience in the Nineteenth Century: Essays toward an Interdisciplinary History of the City* (New York, 1981), 487.

[25] Lester D. Stephens, "The Uses of History," in Vaughn, *Vital Past*, 103. The essays in this collection, which were mostly written before the mid-1970s, stress history's educative purposes and aesthetic value or remind us of James Bryce's dictum that the chief practical use of history is to deliver us from plausible historical analogies.

[26] Arthur M. Schlesinger, jr., "The Inscrutability of History," in Vaughn, *Vital Past*, 318.

[27] Daniel M. Fox, *Health Policies, Health Politics: The British and American Experience, 1911–1965* (Princeton, N.J., 1986), x; Fox, "History and Health Policy: An Autobiographical Note on the Decline of Historicism," *Journal of Social History*, 18 (1985): 349–64.

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To question history's instrumental utility, however, is not to deny the need ordinary people have to seek meaning from the historical record as they confront present-day concerns. According to Michael H. Frisch, "The relationship between history and memory is peculiarly and perhaps uniquely fractured in contemporary American life." This observation leads him to a statement of the public historian's task confirmed by all the works under review here: "Repairing [the relationship] needs to be a major goal of a public history concerned with enhancing our ability to imagine and create a different future through a re-use of the past."[28] Frisch has restated in his own terms the hoary argument that rigorous historical study can contribute to intelligent choices among otherwise equally plausible options. But public historians may well prove no more successful than others in convincing a skeptical market that theirs is more than a utilitarian exercise.

Accordingly, one delves into *Thinking in Time* with great expectations. Since the 1970s, Ernest R. May and Richard E. Neustadt have taught a popular and critically acclaimed course, the "Uses of History," in Harvard University's John F. Kennedy School of Government. Both scholars have made substantive contributions to the literature of public history. May's *"Lessons" of the Past*, a collection of essays on the use and misuse of historical analogy in twentieth-century American foreign policy, remains a classic of the genre. Neustadt collaborated with a public health professor to prepare a fast-paced and lively *and* officially sanctioned account of how government scientists and senior members of the Ford administration mishandled the swine-flu scare. The narrative abounds in blunt judgments intended to admonish and guide their client, Secretary of Health, Education, and Welfare, Joseph A. Califano, as he ran one of the nation's largest and most unwieldy bureaucracies.[29]

Neustadt and May set modest goals. Because most high-level decision makers in government and politics have little understanding of the past they like to invoke, the authors doubt that traditional claims for history's usefulness in policy making carry much conviction. Neither do Neustadt and May maintain that a knowledge of historical specifics alone can "substitute for (even though it supplements) the kind of mental quality that readily connects discrete phenomena over time and repeatedly checks connections."[30] *Thinking in Time* hopes to bring about some slight improvement in the way busy people define their concerns and come to their conclusions. Eschewing grand theory and complicated methodologies, Neustadt and May offer quick, simple rules of logic in a set of "mini-methods." Their advice enjoins policy makers to draw analogies prudently, filling out the time line with a journalist's eye for detail and assigning odds on the likelihood that their story tells what actually happened. It is important, they claim, to separate what is "known" from what is "unclear" and from what is "presumed" at any given time, as well as to specify similarities and differences in seemingly comparable situations.

[28] Michael H. Frisch, "The Memory of History," in Benson, *et al.*, *Presenting the Past*, 6.

[29] Ernest R. May, *"Lessons" of the Past: The Use and Misuse of History in American Foreign Policy* (New York, 1973); Richard E. Neustadt and Harvey Fineberg, *The Epidemic That Never Was: Policy Making and the Swine Flu Affair* (New York, 1983).

[30] Neustadt and May, *Thinking in Time*, 252.

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Neustadt and May ask some basic questions: What newly uncovered facts would change a presumption? How might they challenge connections and alter sequential links in the story line? Finally, when "seeing time as a stream," how do new patterns emerge, and how do interactions between key actors and organizational contexts differ? Investigating these and other subjects, the authors emphasize, may not only expose biases in one's own thinking but also provide insight into the preferences and actions of other decision makers.

Thinking in Time will inevitably disappoint some public and academic historians, who will sniff at the book as a mere exercise in codifying common sense. Others will say (with mixed feelings) that the spirit of Carl Becker inspires Neustadt and May: "Mr. Everyman does not wish to learn the whole truth . . . he wishes to adjust himself to a practical situation and on that low pragmatic level he is a good historian precisely because he is not disinterested."[31] I suspect that Neustadt and May basically want to help decision makers pose "enlightening" questions. Putting a premium on the historical knowledge most people already have, they try to increase the possibility that such knowledge can be tapped for intelligent choices. Attention to such matters informs their approach to history. What at first reading appears simplistic seems on reflection sophisticated in conception and elegant in execution.

Consider Neustadt and May's account of the legislation passed in 1983 to reform Social Security, one of two episodes they characterize as "success" stories in the use of history by policy makers.[32] To grab the reader's attention, Neustadt and May ruthlessly abbreviate their discussion of financial and actuarial matters. They tell a story replete with anecdotes, important details about key actors, and editorial asides. At the same time, they show how readily advantages accrue to policy makers who are neither intimidated by analogies from the past nor entrapped by the rhetoric of the moment. Leaders profit most, say the authors, when they understand just enough about the history of the issue to know what is and is not negotiable, given their own immediate and future interests and those of others involved at the bargaining table. Not all relevant—not even necessarily the most important—themes and data are culled from the historical record. But the material presented is rich enough to spark heated debate. The reader is provoked to doubt the adequacy and reliability of the information typically available in policy circles. The instructional guides accompanying these case studies, moreover, are so well prepared that nearly anyone could adapt them to a variety of educational settings.[33]

[31] Carl Becker, "Everyman His Own Historian," in Vaughn, *Vital Past*, 28.

[32] For a different interpretation, see Paul Light, *Artful Work: The Politics of Social Security Reform* (New York, 1985). The case studies on Social Security listed in the appendix of *Thinking in Time* pursue a somewhat different tack. Students are asked to contemplate the present-day pertinence of legislative particulars in the original act and its pivotal 1939 amendments in order to appreciate the long-term significance of population aging, shifting dependency ratios, and stagflation on the solvency of trust funds, and to challenge the assumptions about Social Security financing made by prominent economists across the political spectrum.

[33] I am indebted to Ernest R. May for sending me nearly all of the Harvard teaching materials for use in preparing this review.

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Writing history in accordance with the prescriptions articulated in *Thinking in Time* will require greater collaboration between public and academic historians. Bridges already exist. The Organization of American Historians publishes a regular column, "Perspectives on Public History," in its newsletter; the *Journal of Social History* and the *Radical History Review* have allocated considerable space to promote new approaches.[34] By way of reciprocity, the *Public Historian* reviews major works of traditional history. The American Association for State and Local History recently invited a distinguished group of social historians to describe the uses public historians might make of the latest research in their field.[35] The time has come, however, to develop a more rigorous agenda of research that will nurture a symbiotic relationship between the two. To the extent that public historians typically turn to the recent past as a point of reference, more traditional historians might aid the mutual cause by stressing the longer view, especially in analyzing structures and processes that took shape during this century.[36] To the extent that public history researchers seek to integrate social, political, economic, and cultural factors in their search for a usable past, their efforts at synthesis should help to counterbalance the Balkanization of scholarly enquiry endemic to the profession at large.[37] Hence "public" and "traditional" historians alike should take their cues from scholars who are calling for the incorporation of political history into existing models for writing social history.[38] More than is currently fashionable, they should work to establish a historical time line that provides an empirical basis for explicating (even if it cannot resolve) current policy conundra and for illuminating the dynamic set of personal and collective values and organizational constraints that give shape to contemporary culture.[39]

THE BOOKS UNDER REVIEW SERVE AS ROAD MAPS that will help the next generation of public historians measure how far their predecessors had gone by the mid-1980s in developing new roles for professionally trained researchers and teachers. Yet none of these books take us very far down byways that might ultimately prove to be interesting to travel. The fresh, more rigorous agenda that academic historians should pursue in order to follow Neustadt and May's path to new vistas is open

[34] See, for instance, the special issues on "Applied History" and "Social History of Medicine," respectively, in the *Journal of Social History*, 14 (Summer 1981); and 18 (Spring 1985). As noted above, earlier drafts of some of the essays in *Presenting the Past* first appeared in the *Radical History Review*.

[35] James B. Gardner and George R. Adams, eds., *Ordinary People and Everyday Lives: Perspectives on the New Social History* (Nashville, Tenn., 1983).

[36] Ronald Steel, *Walter Lippmann and the American Century* (New York, 1981); James T. Patterson, *America's Struggle against Poverty, 1900–1980* (Cambridge, Mass., 1981); and Morris Janowitz, *The Last Half-Century: Social Change and Politics in America* (Chicago, 1978), are appropriate models for what I have in mind, as is Michael R. Beschloss's forthcoming study of the twentieth-century presidency.

[37] Thomas Bender, "Wholes and Parts: The Need for Synthesis in American History," *Journal of American History*, 73 (December 1986): 120–36; and Noel J. Stowe, "The Promises and Challenges for Public History," *Public Historian*, 9 (1987): 47–57.

[38] William E. Leuchtenburg, "The Pertinence of Political History: Reflections on the Significance of the State in America," *Journal of American History*, 73 (December 1986): 585–600; and Peter B. Evans, Dietrich Rueschemeyer, and Theda Skocpol, eds., *Bringing the State Back In* (New York, 1985).

[39] See, for instance, Lawrence Tribe, "Policy Science as Ideology," *Philosophy and Public Affairs*, 2 (1972): 66–110. For a work of revisionist history that can serve as a model for such endeavors, see Terrence J. McDonald, *The Parameters of Urban Fiscal Policy* (Berkeley, Calif., 1986).

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to negotiation. Just as it is folly to suppose that the recent scholarship published by women's historians inexorably points to one and only one set of policy options and feminist perspectives, so too it would be more than a little presumptuous for me to advocate any single course of action or menu of dissertation topics in the current elastic market for public history. The work to date hardly represents the eventual state of the art in either its manifestations as history for the (mass) public or as policy-oriented historical research.

Greater, not less, boldness and diversity are vitally important. If professional historians are to play an active part in developing multidisciplinary projects that meld the talents of various specialists, then public and academic scholars must cooperate to craft a working vocabulary and to refine quantitative and qualitative methods that can be applied in the pursuit of "big" questions that defy preemptive bids by self-proclaimed guardians of virtuosity. And if professional historians truly want to reach broader audiences, they should take the risk of translating research in the humanities and social sciences for various publics in contemporary society. Should they do so, we will find more "public" historians based in biomedical institutes of human development, in radical and neoconservative foundations, and in advertising agencies. We can expect their colleagues to establish liberal arts programs for adults who need to retrain in order to prolong their careers or who want to take greater advantage of the leisure time at their disposal.[40]

The future of public history is ours to imagine and create. The field will flourish if it takes seriously the power of ideas in human interactions and societal developments, past and present. In coming to grips with the challenges and responsibilities that are inherent in being intellectuals, public historians will more fully realize the venerable ideals that inform all first-rate historical narrative and criticism, thereby emboldening their colleagues who labor in more traditional professional settings to do likewise.

[40] Report of the Commission on the Humanities, *The Humanities in American Life* (Berkeley, Calif., 1980); Matilda White Riley and John W. Riley, Jr., "Longevity and Social Structure," in Pifer and Bronte, *Our Aging Society*, 53–77.

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Reviews of Books

GENERAL

REINHART KOSELLECK. *Futures Past: On the Semantics of Historical Time*. Translated by KEITH TRIBE. (Studies in Contemporary German Social Thought.) Cambridge: MIT Press. 1985. Pp. xxvi, 330. $25.00.

This admirably translated collection of essays makes available in English the work of one of Germany's most distinguished philosophers of history. Reinhart Koselleck is an editor of the monumental *Geschichtliche Grundbegriffe* (an encyclopedic dictionary of historical concepts) and a cultivator of the field of "conceptual historiography" (*Begriffsgeschichte*), which, as Keith Tribe tells us in his useful introduction, "emphasizes the importance of linguistic and semantic analysis for the practice of social and economic history" (p. xiii). In these essays Koselleck turns the procedures of "conceptual historiography" to the study of the concept of history itself and, in the process, provides original, erudite, and illuminating insights into concepts that have informed the modern idea of historical being: event, chance, temporality, representation, movement, change, progress, perspective, revolution, modernity, the past, and so on. In contrast to the "history of ideas" approach to social and cultural history, which stresses the evolutionary continuity of certain "unit ideas," Koselleck's procedure permits him to trace radical mutations in the ways that modern Western societies have conceived of their "experiences of history."

The ways in which a society conceptualizes "the past," he argues, set limits on what can be envisioned as its future possibilities and, as a consequence, have a direct bearing on the determination of what can pass for a "realistic" notion of present courses of social, political, and economic action. This is well known and hardly contestable, but Koselleck's use of this topos permits him to revise the received account of the development of the concept of history in the modern era. He maintains that three radical breaks have occurred in the development of the modern idea of history: that of the period of the Renaissance, that of the period of the late Enlightenment and French revolution, and that of the period beginning about 1945.

The notion of "making history" provides an example of such discontinuity. "To say that someone 'makes' history," he writes, "is a modern usage which could not have been formulated before Napoleon or . . . the French Revolution" (p. 200). This is also true for the concept of "history." Before the late Enlightenment the term "history" denoted not a general domain or level of human existence but rather a congeries of discrete chains of events, each of which had a specific set of "stories" (*historiae*) that could be told about it. After the revolution, however, "history" referred to the world of "civilizations" in general and to the processes of formation, expansion, and contraction of the European nation-states: "One of the conceptual achievements of the philosophy of the Enlightenment was enhancing history into a general concept which became the condition of possible experience and possible expectation" (p. 200).

Implicit in the concept of history that informed the dominant ideologies of the period between ca. 1750 and ca. 1945 were the notion of "revolution," considered not as a return to an origin but rather as an opening out onto a future of unlimited, unknowable, and unpredictable possibilities; the sense of the radical acceleration in the tempo of change; the idea that "society" rather than "politics" was the true "subject" and "problem" of history; and the conviction that "revolution" was a world-historical phenomenon, the very motor of "history." All of this constitutes a conceptual precondition of what has come to be called "modernism." Thus, "history" (as we moderns have come to understand it) and "modernity" appear to be a conceptual pair, the dissociation of which reflects, when it does not cause, the condition that we have come to experience as "postmodernism." This thesis will attract the attention of philosophers of history, literary and social theorists, and historians interested in such matters—and will make this a book to be pondered, argued

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about, and either embraced or rejected for some years to come.

Along with the recent work of Paul Ricoeur, Stephen Bann, Sande Cohen, F. R. Ankersmit, Dominick La Capra, and Paul Veyne, Koselleck's work augurs a new era in the conceptualization not only of what "history" means to Western culture but also of what Western culture means for "history."

HAYDEN WHITE
University of California, Santa Cruz

RUDOLPH BINION. *Introduction à la psychohistoire*. Foreword by LOUIS CHEVALIER. (Collège de France, Essais et conférences.) Paris: Presses Universitaires de France. 1982. Pp. 79. 40 fr.

In this book consisting of four lectures given at the Collège de France, Rudolph Binion describes his previous work and reflects on psychohistory. The lectures proceed from a definition of psychohistory to a discussion of psychobiography and the psychohistory of groups (as Binion calls it) and conclude with some thoughts on future directions.

The defining characteristic of psychohistorical method for Binion lies in what Henri Bergson called intuition. For his French audience, Binion perhaps makes more of the Bergsonian origin of intuition than he would have otherwise. He also uses the term "empathy," meaning an intensive and disciplined use of the self, forced on the psychohistorian by the nature of the enterprise. Psychohistory does not replace history but rather appropriates its legitimate domain as defined by its method. It asks the question why and pursues the answer psychologically. One can hardly ignore the traditional sources of history, its objective record. Binion stresses that intuition or empathy can only begin after immersion in the sources. But psychohistory is distinctive in its unabashed use of the self to probe the psychological depths. Intuition, in Binion's formulation, does not then itself provide final answers but must be tested and tested again in the record, using the objective criteria familiar to historians. But without intuition or empathy we cannot talk of a psychohistorical method.

Binion illustrates his understanding of psychohistory by describing his own work, including studies of Lou Andreas-Salomé, Leopold III of Belgium, and Hitler. It is an impressive body of work that has not been sufficiently appreciated. *Frau Lou* remains a fascinating psychological study; the much shorter study of Leopold, though overly intricate, is still important; and Binion's study of Hitler is one of the most productive psychological attempts to date to empathize with and understand an evil, and odd, historical figure.

Nevertheless, a distressing similarity marks Binion's basic explanation of the behavior of otherwise quite different people and situations. In each case the decisive factor turns out to be a childhood trauma that, in Freudian terms, requires repetition, which is manifested in historical events. The psychohistorian seeks to find the original trauma. Hitler, for example, according to Binion, reacted traumatically to his mother's breast cancer and specifically to the manner in which she was treated by a Jewish doctor. The direct evidence for such a trauma is, of course, lacking; Binion's ideas regarding Hitler have incited a noisy controversy. To be sure of anything so remote in time and space is difficult, and part of the problem is the very authority, indeed certainty, of Binion's formulations.

But the controversy over Binion's theories of trauma and its significance has obscured the subtlety of his empathic analysis of his subjects' mental processes, at which Binion is at his best. He is particularly good on the language or rhetoric of Hitler's discourse. However, Binion's discussions of group behavior and attempts to explain the motivations of the German people are problematic. Once again, trauma was decisive, in this case the effects of losing World War I. Binion then explains group behavior through converging and mutually reinforcing traumas.

Binion is a careful thinker and writer. His statements on psychohistorical method, especially on intuition and empathy, are original and not usually given due credit. His interpretations of important people and events in modern history represent a significant contribution to scholarship. These lectures are the best short introduction to Binion's work.

CHARLES B. STROZIER
John Jay College
City University of New York

PEKKA SUVANTO. *Marx und Engels: Zum Problem des gewaltsamen Konflikts*. (Studia Historica, number 18.) Helsinki: Suomen Historiallinen Seura. 1985. Pp. 255.

In this study of Karl Marx's and Friedrich Engels's theory and practice Pekka Suvanto sensibly takes the position that their views on violent conflict cannot be grasped as formulas or principles from which they derived a consistent policy. Rather, their ideas and actions developed in conjunction with varying historical situations and the relative importance they assigned to different problems. This approach allows Suvanto to elucidate the views of Marx and Engels in an informative way without unduly forcing their words into a dogmatic schema. This meth-

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odology is to be encouraged, because much of the literature on the two suffers from the very dogmatism that commentators have striven to find in Marx and Engels themselves.

Suvanto argues that violent conflict first became a problem for Marx and Engels in the theoretical account of social change that they developed at length in *The German Ideology*. In this work war played an independent role in revolutionary social change, which they ascribed to contradictions between forces and relations of production. In this view armed struggle was a consequence or reflection of other forces in society rather than an explanatory factor in its own right.

Actual revolutionary violence in 1848 forced Marx and Engels to come down from this somewhat Olympian position and to consider the exigencies of armed conflict more closely. In particular they had to formulate a strategy for proletarian political forces (with whom should they ally and when?) and recommend tactics (how much force and where?).

International conflicts and wars in succeeding years posed pressing questions for Marx and Engels of a more broadly political rather than specifically revolutionary character. For instance they took a strenuously anti-Russian position, since they saw the autocracy as deeply reactionary compared with the capitalist nations that they somewhat incongruously supported in their journalism. The possibilities for peaceful revolutions in the United States, Great Britain, Holland, and France, as these suggestions appeared in Marx's and Engels's works, are surveyed by Suvanto, as is Engels's famous preoccupation with military science and its employment, by implication anyway, in proletarian struggles.

In Marx and Engels Suvanto can find neither a clear theory of the origin of human aggression nor a principled distinction between armed conflict and terrorism. Instead, Marx and Engels more closely attended to the theoretical delineation of differing economic interests in society. They conceptualized these as class struggle, and Marx backed this up with an almost inverted version of contemporary political economy. Both were unmilitaristic in their basic approaches to politics and even to revolution. They formed no disciplined organization for armed struggle, nor did they conceive of revolution or even the dictatorship of the proletariat in terms of popular force organized in a militaristic way.

The popular force and proletarian dictatorship that Marx and Engels envisaged were composed of an armed people organized in a nonelitist, democratic manner. This form of organization was only loosely and generally presented. Suvanto's category of violent conflict covers everything from popular violence to set-piece international warfare without sufficiently distinguishing the kind of armed political force that Marx and Engels somewhat naively encouraged.

TERRELL CARVER
University of Bristol

MICHAEL W. DOYLE. *Empires*. (Cornell Studies in Comparative History.) Ithaca: Cornell University Press. 1986. Pp. 407.

The analysis of the causes and patterns of imperialism has long been a difficult academic exercise. Expansion is an old activity, the participants in it have often been as confused as varied, and the defined purposes for which it has been undertaken are frequently at variance with the outcome. To structure this far-ranging phenomenon and arrange its course in a concise, interpretive essay takes pluck, if a good adjective from the derring-do novels of empire may be used here. Michael W. Doyle had that pluck and has succeeded remarkably well in his task.

This is a splendid essay, an effective combination of broad historical analysis and well-presented theoretical assessments derived from the social sciences. The book will no doubt stand as one of the best contemporary syntheses of the progress of imperialism. Process and results, action and act are reviewed from the Delian League to the "new" imperialism of the nineteenth century.

Doyle's is a diagnostic approach, a sort of anatomical examination of that fitful organism that has issued from many nations. Between the major parts, metropole and periphery, he finds necessary "transnational contact," the institutions and activities that allow and encourage the movement outward. He follows the varying relationship of these elements through time and place, in different state developments. All of this is done briskly and yet with critical attention to the thoughts of the previous generations of would-be philosophers of imperialism. The result is a volume that commands attention.

Two chapters are unusual in their artful conciseness: that on Rome and that on Great Britain. The assessment of Roman imperialism is a clear synthesis of the best recent work on the subject and yet more, for it has those fine touches that could only come from the appropriate inclusion of Tacitus and Gibbon. The chapter on Great Britain as a modern imperial power is a well-constructed mosaic that will be pleasing to the critical eye of any scholar in the field.

Doyle has read widely and well. He has mastered his material and has done with it something masterly: he has made the whole more than the sum of

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the parts. What follows next from the lively mind of this scholar will be pleasantly anticipated.

RAYMOND F. BETTS
University of Kentucky

RUDIGER JOPPIEN and BERNARD SMITH. *The Art of Captain Cook's Voyages*. Volume 1, *The Voyage of the* Endeavour, *1768–1771*; volume 2, *The Voyage of the* Resolution *and* Adventure *1772–1775*. New Haven: Yale University Press for the Paul Mellon Center for Studies in British Art. 1986. Pp. xv, 247; xiii, 274.

Captain James Cook's ship the *Endeavour* had scarcely docked after its first voyage to the South Pacific in 1771 when published details of the journey found a ready audience in England and abroad. Two centuries hence, that same curiosity for uncharted islands, for white people's intercourse with strange aboriginal cultures continues to thrive. Fascination with Cook's explorations has been aided immeasurably by the many surviving journals, diaries, sketches, and watercolors that present endless avenues for scholars to pursue and make the literature of those three voyages remarkably diverse and extensive.

The latest contribution is an extremely thorough cataloguing of the artwork from Cook's journey by art historians Rüdiger Joppien from Germany and Bernard Smith from Australia. Each volume contains an extensive compilation of illustrations made during Cook's first two voyages, the majority of which come from the British Library. The authors have uncovered some fascinating details about the execution and purpose of the drawings and paintings made during the voyages and the various prints published afterward. The most compelling part of the text is the story of the adventurers themselves: the ill-fated young Sydney Parkinson, who died during the first voyage, never to reap the benefits from publication of his finely executed studies; his shipmates Herman Spöring and Alexander Buchan, amateur artists who also succumbed to tropical diseases on the first voyage; the facile William Hodges, a professionally trained landscape painter who produced a remarkable artistic record of the second voyage; the indomitable scientist Joseph Banks, who supervised the artists on the first voyage and may have created some rather primitive drawings of his own; the naturalist Johann Reinhold Forster, Banks's replacement on the second voyage, who along with his son George contributed watercolors and sketches to the collection.

In the chronologically arranged text that constitutes the first part of each volume, Joppien and Smith have carefully placed each artist—whether professional or amateur—within the larger spectrum of eighteenth-century art. The authors call our attention to the way most strove to avoid the stereotypical portrayals of natives that had been prevalent in Europe since Columbus's discoveries. Although Joppien and Smith carefully trace each known episode of artistic documentation as the ships arrive at and depart from each destination, one wishes they had enlivened their facts with a more colorful account of the adventure. By their own admission, the authors avoided such digressions by considering their work to be the artistic supplement to the journals and biography published over the last few decades by the late J. C. Beaglehole. Thus, the strength of the enterprise lies in the thoroughly researched catalogues, which, unfortunately, follow the text in their chronological arrangement, hindering examination of the work of any one artist in its entirety. Because of practical necessity and the existence of previous publications, the authors chose to omit most studies of fauna and flora and limit their catalogue to landscapes and detailed studies of the natives and their artifacts. A few of Parkinson's beautiful watercolors of tropical vegetation would have added needed variation to the illustrations and indicated more accurately the full scope of the artwork that emerged from the voyages.

These shortcomings do not detract from the masterly research that went into these two elegant volumes. Indeed, the literature on Cook and on eighteenth-century art has been enhanced by the efforts of Joppien and Smith, and I look forward to the appearance of volume 3, which will complete the series.

ANN UHRY ABRAMS
Georgia State University

JORG FISCH. *Hollands Ruhm in Asien: François Valentyns Vision des niederländischen Imperiums im 18. Jahrhundert*. (Beiträge zur Kolonial- und Überseegeschichte, number 34.) Stuttgart: Franz Steiner. 1986. Pp. xv, 166. DM 38.

François Valentyn's *Oud en nieuw Oost-Indien* was published between 1724 and 1726 in five fat folio volumes. In it Valentyn described the Asian places where the Dutch East India Company traded during the seventeenth and eighteenth centuries, from the Cape of Good Hope to Japan, from Persia to the Bandas. He described their landscapes, fauna, flora, religions, governments, laws, histories, and of course their peoples. It is, as Jörg Fisch asserts, the most extensive European work on Asia published before the nineteenth century.

All this, says Fisch, makes it an ideal case study of the way in which Europeans described the various peoples they encountered during the centuries of expansion. In his introduction he perceptively analyzes the problems involved in describing the "na-

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tional character" of another people and the problems of evaluating such descriptions. The problems are universal, but the centuries of expansion provide an unusual opportunity to study them: Europeans, far from home, suddenly confronted astonishingly different peoples with whom they or their forebears had had no previous contact. Valentyn was such a European. His prejudices, ideals, experiences, and relationship to the peoples he describes are known, and they, Fisch insists, coherently shaped his descriptions and judgments even though his work is largely a compilation drawn from earlier sources, published and unpublished.

Fisch then carefully analyzes Valentyn's observations and judgments about the character, morals, laws, religion, and intelligence of the various Asian peoples and the general standards and criteria for his judgments. He searches out examples of relativity, of exceptions, of allowances for extenuating circumstances or for the stage of a people's development and explains why and under what circumstances Valentyn allowed such considerations to affect his evaluations. He quotes extensively, sometimes repetitively, translating in the text but reproducing Valentyn's Dutch in the footnotes. Not surprisingly, he finds Valentyn's observations about Asians to have been conditioned by his enthusiastic support for the Dutch commercial empire. People directly under Dutch control received more attention and usually harsher judgments than did voluntary allies or independent peoples. His standards were European and Christian, Dutch and Reformed.

Fisch is thorough in his analysis; perhaps more thorough than Valentyn's text can bear. He might have been more thorough, however, regarding Valentyn's sources. Valentyn used a wide range of materials and often reproduced them verbatim, usually without acknowledgement. Despite Fisch's disclaimers, Valentyn sometimes repeated the judgments found in his sources. Because most of them were seventeenth-century Dutch sources, which imply standards similar to Valentyn's own, the problem is rarely serious. But some of Valentyn's sources, for example those describing Formosa, were a century old when he wrote, and some were Portuguese or Jesuit. Fisch may have confused their judgments with Valentyn's. In any case more attention to Valentyn's sources would have improved a generally perceptive and interesting book.

EDWIN J. VAN KLEY
Calvin College

JAMES AXTELL. *The Invasion Within: The Contest of Cultures in Colonial North America*. (Cultural Origins of North America.) New York: Oxford University Press. 1985. Pp. xv, 389.

This is the first of three projected volumes on the cultural origins of North America focusing on the conflict of cultures between the English, French, and Indians in the colonial period. The Spanish are excluded by the rationale of "historical marginality" and on the more justifiable grounds of the expediency of "selectivity" as a "virtuous necessity" (p. x).

The major emphasis of this volume is on the efforts of the French and English to educate, "civilize," and convert the natives to Christianity. In an excellent first chapter James Axtell provides a general view of the basic religious beliefs of the Indians without, however, using a possible alternative of selecting one group for a more detailed description to add specifics to a general discussion. Both French and English went beyond religious efforts and recruited Indians as military allies in their long struggle for superiority in North America.

As a part of the emphasis on "fur, fish, and the faith" (p. 23), French missionary efforts were more successful than English as the Jesuit fathers by 1670 superseded the Recollects (Friars Minor), who viewed the Indians and their culture pessimistically. The Jesuits were capable of enduring rigorous hardships and were committed to persistent efforts based on their experience in America. Striving to convert whole villages and tribes to Catholicism, they adopted the more practical intermediate goals of gaining the support of native political leaders, removing shamans as religious leaders, and encouraging general institutional and emotional support to new converts. Their goal was to recreate the "major institutions of Roman Catholicism—chapels and churches, sodalities and sacraments, calendars and choirs, ceremonies and services" (p. 78). In contrast to the English, the French placed greater emphasis on adults than children.

The competing English approached the North American challenge for hegemony with confidence in "the essential educability of the American natives" (p. 131) and a commitment to the long and arduous task of complete conversion. Eventually directing their major efforts toward youth, they preferred to send Indian boys to English schools and colleges. As described by the author in perhaps the most striking sentence of the book, they were to be "sexually segregated, morally guarded, classically oriented, rigorously disciplined, patriarchically dominated, and, until the eighteenth century, located in English territory, far from the contagion of traditional habits, families, and friends" (p. 179).

These efforts notwithstanding, a significant number of English colonists, prisoners, and traders were converted to Catholicism and preferred to remain as apostates in Canada with the French. Other English living among the Indians chose to continue with the pleasures and freedoms of a primitive society as "white Indians." We must await the author's third

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volume in this series to witness eventual English dominance with whatever social, moral, and material costs that resulted.

This is a well-researched and extensively documented study with a judicious analysis of the interaction between the three major groups involved. Well-organized and skillfully crafted, it richly deserves the recent awards of the Albert B. Corey, Gilbert Chinard, and Erminie Wheeler-Voegelin Book Prizes.

W. STITT ROBINSON
University of Kansas

JEAN-CHRISTOPHE AGNEW. *Worlds Apart: The Market and the Theater in Anglo-American Thought, 1550–1750*. New York: Cambridge University Press. 1986. Pp. xvi, 262. $24.95.

Jean-Christophe Agnew has written an arresting, stimulating book, ambitious in scope and impressingly erudite. Covering the critical phases of early capitalist development, from roughly 1550 to 1750, Agnew explores the changing meaning of the theater and the market in Anglo-American thought, the particular resonance between theatricality and commerce in public discourse. Beginning with the marketplace of the ancient and medieval world, the author draws on the anthropological perspective of Victor Turner and others to show that the market was a strictly defined liminal space where public barter was visibly conducted and officially regulated and where society's rules and aspirations were symbolically reaffirmed and periodically challenged. In the course of the sixteenth century this dense network of markets gave way to a more undifferentiated market process and to the increasing liquidity and impersonality of exchange relations. The disruptive and transformative power of commodity exchange prompted new anxieties about social identities that were registered in the new realism of Tudor literature, in character books, cony-catching pamphlets, and courtesy manuals. But the phenomenology of market capitalism was best captured on the Renaissance stage, Agnew argues, where conventions such as the soliloquy and the aside heralded a new reflexivity in the dramatic idiom. Thus, a new vision developed of the *theatrum mundi*, no longer a divine commentary on human vanity but a secular commentary on the plasticity of human relations.

This deconstructive exercise proved too subversive to Puritan thinkers, who sought to reorder their world through faith, work, visible sainthood, and covenant theology. With the suppression of the theater in 1642 the stage lost its privileged position as a medium capable of invoking the anxieties that surrounded the crisis of representation. Restoration drama lacked the reflexivity of its predecessor. It adopted the persona of a coffee-house spectator, relishing the comedy of manners that capitalism brought in its train without philosophical introspection. During the early eighteenth century, spectatorship reached its apotheosis, reflecting a more settled, if cynical, vision of capitalist relations. The philosophical defense of capitalism came with the Scottish school, particularly Adam Smith, whose notion of sociability incorporated the histrionic and rendered it serviceable to the free market ideal. Market roles socialized humanity. Sympathy, like material need, became determined by the imperatives of the market.

My one major reservation with this remarkably rich and original book is its idealist character. Given the comparatively slow development of capitalism in English society and the fact that a market fully based on the exchange of commodities did not emerge until the eighteenth century, one wonders whether the crisis of representation was always so critical as Agnew suggests and whether it was not, at specific moments, overlaid by political and religious factors. This is recognized by Agnew (p. 129), but it is not sufficiently developed. Similarly little attention is given to those interstices in the social structure at which the problem of social identity was most acute. For example, a closer look at the particular modes of social reproduction among the greater gentry, for whom marriage, land, and political office were so crucial to dynastic continuity, would explain why the lure and volatility of commercial wealth and the calibration of social worth were such abiding preoccupations. The function of London as a center of consumption and preferment within an aristocratic, capitalist order would clarify that city's obsession with rank, status, and social solidity and how, with the financial revolution of the post-1688 era, new rules of civility emerged. In Agnew's account of capitalism its different rhythms and trajectories remain unspecified. One is always left with a reductionist formulation: the ineluctable and ubiquitous pressures of the market.

That should not detract from the importance of this book. It is elegant, original, expansive. A very impressive monograph by a sharp intellect, it should be read by everyone with an interest in the major social themes of early modern culture.

NICHOLAS ROGERS
York University, Toronto

JAMES MICHAEL HILL. *Celtic Warfare, 1595–1763*. Edinburgh: John Arnold: distributed by Humanities, Atlantic Highlands, N.J. 1986. Pp. xi, 203. $29.95.

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This learned and readable monograph has a clear thesis: the continuity of a specific kind of warfare fought by Celts in Great Britain and North America in the sixteenth through the eighteenth centuries, a kind in fact reaching back to Roman times (Julius Caesar is in the bibliography). Because the dominant Celtic characteristic was the wild charge, weapons whirling, voices screaming, and appearance vivid, readers can be sure of much enjoyment. And, because James Michael Hill has sought to confirm this theme with extensive archival research, I can but praise the enterprise. Need I add that such praise must be accompanied by strong expression of caution? For, although Hill has found what he looked for and will thus win applause from Celticists, he has ignored such factors as conflict of religion and class. He belongs with those nationalist historians, and particularly with the German "Kulturgeschichte" writers, on whom most of us have built.

Hill's first two chapters sketch the historiography and the pre-1595 history of the subject. Both chapters show extensive reading and serious thinking, but the dominant theme, Celtic-Saxon relations, has to be developed as a potential for cooperation frustrated by Saxon imperialism. The context of Anglo-Saxon survival in the face of Roman and Norman imperialism is not mentioned.

The main chapters are essays on six wars: late Elizabethan Ireland (1595–1601), Scotland in 1644–47, Killikrankie, the risings of 1715 and 1745, and the French and Indian wars in North America. Half of his eight-page conclusion, moreover, is devoted to the predominance of Celtic population and warfare in the South during the American Civil War. This enables Hill to fulfill his promise without overburdening his text.

Though a professional historian, Hill is not an established scholar in this field, so he is not drawing on previously published researches of his own. He uses sources less to revise previous views than to illustrate his thesis. The references to modern scholarship are relatively light and uncontroversial, although he acknowledges his debt to fellow Celticists. The book seems to be an extension backward in time of Grady McWhiney's work on the American Confederate forces. Perhaps because of this, the consultation of unprinted material dwindles as Hill approaches modern times. Hence, in his chapters on the seventeenth century, archival sources and secondary writing get short shrift. Also, significant modern Canadian writing on the French and Indian wars is overlooked. We shall all be interested to see how this likeable, enthusiastic narrative fares in the hands of the specialists. That it belongs to an important revival of race- and nation-oriented historical literature can be of little doubt.

M. R. POWICKE
Toronto University

ANTHONY J. BARKER. *Captain Charles Stuart: Anglo-American Abolitionist.* Baton Rouge: Louisiana State University Press. 1986. Pp. xi, 328. $32.50.

Despite his influential position in the antislavery movement on both sides of the Atlantic, the colorful and eccentric Charles Stuart has never before been the subject of a full-scale biography. It is not hard to understand why, for his is a singularly difficult life to capture. Most daunting has been the paucity of sources. Almost nothing is known of Stuart's childhood (Anthony J. Barker is the first historian accurately to fix the place of his birth, Bermuda), and even for his adult years the extant evidence is often distressingly sparse. Not even an authentic portrait survives to show what this "handsome," "iron-framed" man looked like. The striking asymmetry of Stuart's career, moreover, creates special problems for an artful biographer. Retired on pension from the Madras army of the East India Company at the age of thirty-four, he first married when he was seventy-one. Only in middle age did he become an abolitionist, and his chief contributions to that cause were packed into less than a decade. The last quarter-century of his life he spent in bitter isolation from friend and foe alike.

To his credit, Barker has substantially overcome such problems and produced a fascinating biography, one that renders comprehensible a quirky and complex being and sharpens our understanding of the Anglo-American crusade against slavery. Without unduly inflating his subject's importance, Barker convincingly demonstrates that Stuart's contribution to abolitionism went far beyond his well-known recruitment of Theodore Weld. In Great Britain Stuart became a key figure in the provincially based campaign for immediate emancipation almost from the moment he embraced the antislavery cause in 1830, helping to convert first the antislavery establishment in London and then Parliament itself. Through his correspondence with the young Weld he brought the central issues of American abolitionism to the attention of British reformers, and by his attacks on the American Colonization Society he paved the way for William Lloyd Garrison's reputation-enhancing visit to England in 1833. Later, after the technical destruction of West Indian slavery, Stuart played an important if secondary role in the fight against black apprenticeship.

For a time, at least, Stuart also had an impact on the American scene. His well-publicized assault on colonization, together with his antislavery best seller, *The West India Question*, earned him a position equal to that of George Thompson as a representative of transatlantic abolitionism. That position was further reinforced when in 1834–35 the peripatetic Stuart served with conspicuous bravery and resourceful-

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ness as an agent of the American Anti-Slavery Society.

Stuart was in many ways a "reckless misfit," a man of "disruptive talents" whose eccentricity stood out even in a movement replete with odd characters. By 1840 his stubborn resistance to the changing currents of abolitionism and a growing fixation on scriptural orthodoxy (which eventually alienated even such comrades as Weld and Gerrit Smith) had started Stuart down the road to obscurity. Yet, as Barker perceptively demonstrates, even Stuart's shortcomings illuminate the history of the movement to which he committed the best years of his life, his very rigidity helping to chart the shifting means and ends of Anglo-American reform. Students of transatlantic abolitionism will profit greatly from this thoughtful, well-crafted biography.

RICHARD H. SEWELL
University of Wisconsin

MICHAEL HANAGAN and CHARLES STEPHENSON, editors. *Proletarians and Protest: The Roots of Class Formation in an Industrializing World.* (Contributions in Labor Studies, number 17.) Westport, Conn.: Greenwood. 1986. Pp. viii, 250. $45.00.

Michael Hanagan and Charles Stephenson have provided the useful service of bringing together articles on labor history spanning the First and Third Worlds. To wonder why the Second World is absent from a collection that already surpasses the usual academic boundaries might seem churlish, but a problem arises that is almost inescapable in the medium. A book of essays intended to focus on broad questions and large processes can rarely cover the ground comprehensively or answer the questions it poses. This collection tantalizes its readers and encourages them to look beyond their geographical confines. An intelligently presented annotated bibliography is provided to help them along.

The introduction by Hanagan and Stephenson falls uneasily between a presentation of the individual essays and theoretical statement. They identify several questions from the essays. Where did the work force come from? How did it get to the industrializing city? What did it find there? They are asking, almost, for a complete history of the origins of a proletariat, and that neither the introduction nor the volume as a whole achieves much focus is no wonder. Although they say that "the essays portray class formation as a series of social solidarities that bind workers together and mark them off from other social groups," they immediately contradict themselves by turning to the interpenetration of rural society and rural ideologies with growing working classes (p. 4). In fact, an appreciation of the varying forms that these interpenetrations take—within Europe as much as outside it—is the most important contribution of these essays as a whole. The complexities that emerge belie the editors' attempt (especially on page 18) to define a dichotomy between "core" and "noncore" nations.

The two gems of the collection come from Latin Americanists. Looking at the Amazon rubber trade from 1850 to 1920, Barbara Weinstein suggests that the conditions that created demand for labor gave laborers means to avoid becoming beholden to the rubber industry. Their mobility and their ability to use relations with planter elites, creditors, and local officials against the foreign rubber companies allowed them to resist capitalist work discipline and work rhythms and to maintain "traditional Amazonian patterns of production and exchange" even during the rubber boom (p. 70). Her article is concise yet rich in detail and clear in its analysis. Florencia E. Mallon looks at copper miners in the Peruvian highlands and shows that the mines did not recruit labor directly but through intermediaries who made cash advances to peasants and thus established a particular kind of relationship with the rural communities—more profound in some ways than the relationship of worker to capitalist. Even as mining companies tried to rationalize their work force in the 1920s, the workers had so closely woven the culture of village and mine together that a specific proletarian way of life did not emerge. Mallon suggests that the intertwining of rural community and mine labor provided workers with both material resources and "a language of solidarity" in their difficult struggles with capital (p. 222). She vigorously argues against conceptions of class formation that posit a "classic" European pattern of proletarianization and define anything short of that as "transitional" or "immature." This leaves the question of whether the "classic" pattern is so classic in Europe as well.

In a third essay on Latin America William Roseberry explores a case in Venezuela in which the recruitment of an industrial working class did not impinge on a peasantry with age-old traditions but on a peasantry that had only recently emerged. Intelligent as his exploration is, he develops his case through reference to material presented elsewhere, and his essay is less vivid and less convincing than those of Weinstein and Mallon. The final contribution on the Third World is weaker. Sharon Stichter packs a detailed narrative of Kenyan labor history into a short space, so much so that she provides little analysis. At the beginning she posits that migrancy defined a "distinct variety of labor system," with "a particular set of class relationships" (p. 171)—a position too static and rigid to explain the changeable and problematic nature of migratory labor. At the end, she decides that "the worker-peasant alliance in the Mau Mau rebellion was similar to that

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which produced successful Third World revolutions in countries such as Algeria, China, and Cuba" (p. 192). But, in the absence of any serious consideration of the rebels' thought or mobilization, the argument is a tautology: the peasant-worker rebellions were similar because they involved peasants and workers.

The pieces on Western Europe emphasize the varied connections of cities and countryside. As Louise A. Tilly writes, "Industrialization does not happen to whole countries" (p. 127). Her essay compares the regions of Lyon and Milan, and Leslie Page Moch studies the regions of Roubaix and Nîmes. Both examine in interesting ways the nexus of agricultural and rural industry, of migration, of factories and shops, of industry and commerce; both reveal the variability of industrialization and deindustrialization, and they situate—albeit briefly—workers' individual and collective action in this changing context. Hanagan, writing on the Stéphanois region of France, stresses the unevenness of industrialization and the relationship between rural and urban protest. These accounts are a bit breathless at times, but that is a hazard of the genre. Less satisfactory is Flemming Mikkelsen's article on Scandinavia. He amasses enough data to put his region into a pattern developed in reference to Southern and Western Europe: that of growing proletarianization leading to conflict and then to political action. But his article—covering one hundred forty years of the histories of three countries—does not convince the reader that such a conception is particularly revealing. Some of his coauthors, with different sorts of investigations, have suggested that looking at wage labor and industrialization in particular regions provides a picture of workers' behavior that is not only more detailed but in some ways fundamentally different from the linear notion implied by a concept such as "proletarianization."

FREDERICK COOPER
University of Michigan

ANCIENT

A. R. DAVID. *The Pyramid Builders of Ancient Egypt: A Modern Investigation of Pharaoh's Workforce*. Boston: Routledge and Kegan Paul. 1986. Pp. x, 269.

In some respects more astounding than ancient Egypt's ability to erect vast monuments with a relatively primitive technology was the Egyptians' expertise in organizing labor. Whether battalions of "free" peasants brought for limited periods to a work site or gangs of POWs organized on a more permanent basis, the administration displayed great skill in housing, feeding, and caring for this work force. Workers' settlements are frequently mentioned in our textual sources from all periods of Egypt's history and must have constituted a significant factor in demographic patterns in the ancient Nile Valley. In the New Kingdom the cabinet official known as the "king's scribe of recruits," that is, the secretary of labor, with direct supervision of these settlements, became one of the most influential of all royal ministers.

In spite of the undoubted frequency of such labor camps or artisans' towns, few have been revealed through archaeological investigation. The most famous, perhaps, is the village of Deir el-Medineh on the west bank at Luxor (ca. fourteenth to twelfth centuries B.C.), which housed the necropolis workers who labored on the royal tombs in the Valley of the Kings and is famous for its wealth of ostraca, which yield a vivid panorama of contemporary life. Another was unearthed at Amarna, the erstwhile residence of the heretical king, Akhenaten, and a third has been detected at Giza, adjacent to the pyramid of Mycerinos (twenty-sixth century B.C.). A fourth is located at Kahun in the Fayum in close proximity to the pyramid of Senwosert II at Illahun (ca. nineteenth century B.C.). This was excavated by W. M. F. Petrie late in the last century, and the wealth of artifacts and papyri his work produced provides the same illumination for the Middle Kingdom that Deir el-Medineh material gave us for the New Kingdom. Now, after the passage of ninety years, the Manchester Museum, under the direction of A. R. David, is subjecting Petrie's find (only partly analyzed at the time) to a rigorous re-examination; in the present charming work she tells us of the results.

The book takes its place among a growing number of similar works of popular appeal—Morris Bierbrier's and Jaroslav Cerny's on Deir el-Medineh, T. G. H. James's on ancient society, Harry Smith's on Memphis (to name but a few)—which in an earlier day would have been classed under the rubric "everyday life" or the like. David not only tells us of her current research but wisely chooses to use the invaluable corpus of Kahun and Gurob papyri to reveal the whole life of this ancient community—from the law courts to the market place, from education and literature to gynecology and religion.

No one who has visited the sites of Lahun and Kahun, proceeding along the rim of that mysterious ancient dike, can help but be struck by the solitude and romance of the sites. David's work has given these silent ruins a voice, at once informative and hypnotic, whose romantic cadence is in perfect harmony with the atmosphere they evoke.

DONALD B. REDFORD
University of Toronto

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LINDA J. PIPER. *The Spartan Twilight.* New Rochelle, N.Y.: Aristide D. Caratzas. 1986. Pp. xi, 244. $60.00.

This book opens with Linda J. Piper's contention that, despite W. G. Forrest's claim that "no connected history of Sparta is possible" in the Hellenistic period, she has "not only attempted the impossible, but extended it into the Roman period" (p. xii). This attempt, though commendable, is not completely successful. The sources for Hellenistic Sparta are far from satisfactory, and Piper has examined little beyond Polybius, Plutarch, and the occasional information supplied by Diodorus, Pausanias, and other late writers in tracing Sparta's military and political relations with more powerful states. Moreover, the story of Hellenistic and Roman Sparta is neither particularly important nor, save for the social reforms attributed to Agis IV and Cleomenes III, especially interesting.

A brief section on the ancient sources and an introduction lead to a cursory chapter, "Sparta after Alexander," which surveys more than one-half century down to 244 B.C. Then, three chapters discuss the reforms of Agis and Cleomenes at length. Unfortunately, these sections leave much unclarified about the nature of Spartan society in the late third century, and the listing (p. 30) of terms for various classes within that society raises more questions than it answers. Unfortunately, Piper did not read E. David's *Sparta between Empire and Revolution* (1981), before tackling this problem. Furthermore, the use of the term *demos* is not helpful in discussing Spartan society. Spartan history in this period was intimately intertwined with that of the Achaean League, and that the great Achaean statesman, Aratus of Sicyon, a foe of Sparta, comes off less well in Piper than in Polybius is perhaps understandable. The attempt to diminish his reputation, however, is no more persuasive than that to exonerate the Spartan tyrant Nabis, for whom the ancient sources have little good to say. The extremely complicated political and diplomatic relations of Sparta with Rome, Macedon, and the Achaean and Aetolian Leagues from 205 to 146 occupy two difficult chapters. Then, two chapters, "Sparta under Rome, 146–31 B.C." and "The Imperial Period," bring together what little can be gleaned about Sparta in the Roman era. Finally, a chapter entitled "The City of Sparta" describes it as it was depicted by authors of the Roman age.

To have the story of postclassical Sparta told in a single volume is useful, but this is largely a synthetic work, based to be sure on the ancient sources and reflecting the relevant modern literature, but lacking any theme other than the decline of a once powerful and respected state.

CHARLES D. HAMILTON
San Diego State University

B. C. MCGING. *The Foreign Policy of Mithridates VI Eupator, King of Pontus.* (Mnemosyne, Bibliotheca Classica Batava, Supplement, number 89.) Leiden: E. J. Brill. 1986. Pp. viii, 204. f. 80.

Almost a century has passed since the publication of T. Reinach's great biography, *Mithridates Eupator, König von Pontos* (1895), the last book to focus exclusively on the career of the king who fought two major wars against Rome in the first century B.C. and unwillingly contributed to the creation of a new, Roman order in the eastern Mediterranean. In recent decades so many specialized studies have revised Reinach's views that to discover, much less to master, all the bibliography has become no small or easy task, making a critical synthesis of recent work a serious desideratum. B. C. McGing's excellent monograph goes far toward meeting that need. Cautious, thorough, and insightful, it will deservedly be the starting point for future discussions of Eupator.

McGing's subject is broader than the title of his book might suggest; the introduction and the first chapter, which together form almost a quarter of the work, examine the kingdom of Pontus and the careers of Eupator's predecessors on its throne. The second chapter traces the establishment by Eupator of an empire around the Black Sea during the first years of his reign. The king's activities in Asia Minor before the first war with Rome and that war itself are the subjects of the next two chapters. His later conflicts with Rome are treated more briefly in chapter 5. McGing summarizes his main lines of argument in the book in a conclusion and adds two appendixes, the first discussing the vexed question of the royal Cappadocian coins as a chronological source (McGing supports Bono Simonetta's interpretation of the exergual marks as dates but concedes the force of Otto Mørkholm's objections), the second reviewing the ancient literary sources for Eupator's career.

In brief, McGing makes a case for an essentially Roman interpretation of Eupator's intentions at the important junctures in his career. Thus, he maintains that in the 90s "it became increasingly clear that nothing worth while could be achieved in Asia Minor without coming into conflict with Rome" (pp. 169–70) and that the prospect of a "Roman takeover in Bithynia," which McGing proposes as the immediate cause of the Third Mithridatic War, "offered Mithridates the opportunity he was waiting for" (p. 144). Some readers may feel that a more shaded assessment of the king's motivation at different times would better fit the sources, but McGing's arguments in support of his reading are strong and interesting.

Practical considerations probably dictated that McGing relegate close analysis of the evidence con-

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cerning several chronological cruces to journal articles. Understandable as it is, that decision forces readers to look outside the book for arguments central to it. Scholars will want to consider whether McGing's chronology, which underpins his reconstruction of the king's activities, is as secure at several points as alternate reconstructions might be.

DENNIS G. GLEW
Moravian College

C. P. JONES. *Culture and Society in Lucian*. Cambridge: Harvard University Press. 1986. Pp. xiv, 195. $25.00.

After excellent books on Plutarch and Dio Chrysostom, C. P. Jones goes for his hat trick with Lucian. Another book on Lucian? There has recently been a plethora, acknowledged in Jones's prefatory review of the literature. This is certainly not the "fresh portrait" claimed by the blurb, an advertising extravagance that would have seduced one of Lucian's own targets, the ignorant book collector!

Jones's concern throughout is, correctly, to emphasize the contemporary bite of Lucian's satire against those who disparage him as a bookish flogger of dead horses. This comports a rescue operation from his notoriously curious dismissal in the influential *Greek Sophists in the Roman Empire* (1969) of Jones's mentor, Glen Bowersock.

Chapter 1 adumbrates some modern views of Lucian, a sensible reminder that modern experiences condition all perceptions of the past. George Orwell's cynical remark that keeping the past up to date is a full-time job extends into honest reappraisal.

Twelve crisp and elegant chapters discuss Lucian's career, compare him with many other *littérateurs* of the day, and analyze many of his plentiful writings. Jones achieves his view of Lucian by perceptive reading of both the satirist himself and wider sources. The author's well-known epigraphic flair is evident, and he is abreast of other archaeology—a virtue being precisely that he sees these evidences as relevant to Lucian. In addition to clear and rigorous detail, Jones makes some arresting generalizations (for example, that Lucian was the first teller of the sorcerer's apprentice story and the first Greek to describe all the Roman empire as "us" [pp. 48–50, 89]) and witty comparisons, notably that mime was a precursor of ballet and disco-dancing (p. 69).

Three appendixes usefully discuss Lucian's chronology and disputed works. The sixteen-page bibliography is a treasure-house; add perhaps R. Venci's *La presunta conversione di Luciano* (1934), and C.-A. Mayer's *Lucien de Samosate et la renaissance française* (1984). The index is perhaps a trifle frugal but prosopographically reliable. The book is attractively produced and has no serious misprints.

A few details follow. On "Pantheia" or "Panthea" (p. 17, n. 62) compare Dio Cassius 59.11.3. On political interpretations of the *Saturnalia* (p. 87, n. 40) compare the *Dialogue between Rich and Poor* by the Byzantine Alexios Makrembolites, who knew and wrote on Lucian. Aulus Gellius actually has many rather than few dramatic scenes (p. 93, n. 24). "Cocconas" might be an indecent nickname (p. 135; compare *Greek Anthology* 12.222), but possibly the rare patristic verb *kokkuao* (to talk foolishly) is the answer; it would suit the writer Lucian is mocking. Perhaps the only occasion on which Jones's accuracy falters is his review of Lucian's reading (pp. 150–52); Lucian *does* mention Lysias and a number of other Attic orators, a topic that would have benefited from consideration of the contemporary critic Hermogenes of Tarsus, conspicuously absent from Jones's index.

But these are sunspots. Though not always novel, this detailed restatement is needed to combat the still-persistent silly notion of Lucian as an irrelevant bookworm. It should put its opponents to rout, or on the defensive. Certainly, any future writers on Lucian will find it hard to keep up with this particular Jones.

BARRY BALDWIN
University of Calgary

JANE F. GARDNER. *Women in Roman Law and Society*. Bloomington: Indiana University Press. 1986. Pp. 281. $29.95.

Ancient historical writers had little to say about women, other than those who were related to rulers or other significant men; women wrote little, and not usually about themselves. As a result, ancient legal documents provide much vital and otherwise unattainable information about the conditions in which women lived. Law codes and case histories not only define the rights available to women but indicate the full range of activities in which women were involved. As Eva Cantarella has shown (*L'ambiguo malanno* [1981]; English translation *Pandora's Daughters* [1986]), Roman law leaves an impression of men's attitudes toward women that may be more valid than that which can be inferred from literature or philosophy alone. Jane F. Gardner's study of women in Roman law offers an assessment of women's status that compares favorably with more recent societies, such as Victorian England.

Gardner shows how and why Roman women had much greater control over their lives and fortunes than did Greek women before the Hellenistic age. The powers of Roman women's guardians were more closely described in the law; women were able

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to inherit, own, and manage property; judges, rather than a woman's male relatives, determined fault in divorce and distributed property accordingly. Gardner also provides an account of the status of lower-class women and slaves, who served wealthy families or worked as wet nurses, prostitutes, and entertainers. The complex legislation that protected free men and women did not in most respects apply to slaves, who could not contract marriage and had no legal right to keep their offspring, although in practice young children remained with their mothers.

Gardner avoids technical jargon but assumes that readers will know the general outlines of Roman history. Occasionally, she does not explain ordinary Latin terms such as *agnate* (relative on the father's side), and *sctum* (for *senatus consultum*, senatorial decree); sometimes, she concentrates so long on details of interpretation that the main line of argument becomes hard to follow. One wishes also that she had offered the nonspecialist a general survey of the kind of source materials Roman legal historians must work with. But in general Gardner's judgment is sound, and her discussion of controversial issues balanced. Instead of simply reacting to and complaining of legislation that seems to discriminate against women, such as the *lex Voconia* of 169 B.C., Gardner shows that the law was designed to maintain the status quo of male guardianship but not to deprive women of the right to own property.

Even where Roman law and litigation do not reflect actual practice, they at least indicate important issues: individual rights and inheritance (both male and female) and, in all periods, the welfare of the family. To some extent, as always in the ancient world, women of the propertied classes apparently were able to exercise influence through their men, although in the empire not even the men had the power to enforce their own decrees without imperial consent. But what is most surprising, and perhaps disappointing, is how little the status of Roman women altered over the centuries. Although by the end of the fourth century women were able to serve as tutors, with few other exceptions women had acquired no significant new rights or powers since the early days of the Republic.

MARY R. LEFKOWITZ
Wellesley College

PHILIP ROUSSEAU. *Pachomius: The Making of a Community in Fourth-Century Egypt.* (Transformation of the Classical Heritage, number 6.) Berkeley and Los Angeles: University of California Press. 1985. Pp. xvi, 217.

Philip Rousseau makes a significant contribution to the history of early Christian monasticism in this thoughtful, penetrating study of Pachomius (died 346), often called the founder of cenobitic monasticism. Rousseau's approach is avowedly revisionist, seeking to correct what he considers a distorted picture of Pachomius, which was first shaped in the early sources and has been perpetrated by modern scholarship. He does indeed "discover someone with much more to offer than rules, anecdotes, or a 'cenobitic movement' . . . a man unlike the pioneer of general histories—harsh, efficient, the drill-sergeant of the ascetic life, presiding without humor over a small empire of teeming, unreflective communities" (p. xiv).

A brief review does not permit a detailed description of the Pachomius and the Pachomian world that Rousseau unveils as a result of his penetrating reading of the sources, his imaginative interpretation of their enigmatic message, and his skillful condensation of a long tradition of Pachomian scholarship. A skeletal outline of his approach to the historical Pachomius, however, may entice readers to try this excellent book. Rousseau begins with a brief but perceptive chapter surveying broad trends in fourth-century Egyptian society relevant to Pachomius's activity but heretofore little noted in treatments of his career. Then he devotes a chapter to a description of the sources and provides a balanced assessment that breaks no new critical ground but successfully condenses modern scholarly opinion. Rousseau convincingly argues that the complex corpus of surviving texts, especially the *vitae*, provides trustworthy information about Pachomius. A third chapter is devoted to the crucial sequence of events marking the passage of Pachomius from a pagan soldier to the leader of a confederation of monastic communities, a trajectory marked by uncertainties and experimentation leading only gradually to the formation of a unique new form of Christian life. The next four chapters, constituting the heart of the book, seek to portray in detail the pattern of life that emerged in the *Koinonia* during Pachomius's lifetime: the daily routine, the nature of the early rule, the style of leadership, and the spiritual goals that Pachomius sought to realize among his brothers. Then Rousseau devotes an intriguing chapter to the interactions between the Pachomian community and the outside world and convincingly shows that interactions on many levels had a significant impact on both spheres. The final chapter is devoted to some reflections on the transition from Pachomian leadership to that of his successors, a tortured process marked both by continuity and by transformation of the special pattern of life Pachomius had tried to forge.

Across this spectrum of topics, Rousseau always has something fresh and challenging to say. In my judgment he has made Pachomius a more understandable historical figure and has shown the

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uniqueness of the way of life he struggled to define. Many will surely disagree with Rousseau on some issues, especially his interpretation of the character of the early Pachomian rules and his analysis of Pachomian ascetic goals. But those who henceforth elect to discuss Christian monasticism and Pachomius's role in it will do so at risk if they neglect this book. And, when they read it, they will become aware of a man deserving more than treatment "only as a pioneer, quickly superseded in the history of monasticism by men more articulate and more influential—Basil, Evagarius, Jerome, Augustine, or Cassian" (p. xiii).

RICHARD E. SULLIVAN
Michigan State University

MARCIA L. COLISH. *The Stoic Tradition from Antiquity to the Early Middle Ages*. Volume 1, *Stoicism in Classical Latin Literature*; volume 2, *Stoicism in Christian Latin Thought through the Sixth Century*. (Studies in the History of Christian Thought, numbers 34 and 35.) Leiden: E. J. Brill. 1985. Pp. x, 446; x 336. f. 260 the set.

In the two volumes of this massive work, Marcia L. Colish ranges from Cicero to Martin of Braga (sixth century after Christ). Her object is to establish the diffusion and significance, positive or negative, of Stoic philosophy in pagan writers of Latin arranged by genres (satire, epic poetry, history, scholia, grammar, rhetoric, law, and so on) and in the Christian writers of Latin who followed them. Separate bibliographies and indexes help make each volume usable independently, but the reader of volume 2 will need to consult volume 1, especially for the author's introduction to the history of Stoicism as a philosphical school and for her account of its doctrines in physics, ethics, and logic.

An enormous amount of information, including extensive bibliographical notes, is conveyed in a clear and plain style. The book is free of jargon or any fashionable view of intellectual history. At its best, especially on Tertullian, Augustine, and Boethius, it has the virtues one expects from a series of good encyclopedia articles. By the sheer scale of her work, Colish succeeds in demonstrating the presence or absence of Stoic notions in authors who did much to shape the thought of the Middle Ages and early Renaissance. She has improved the perspective for viewing the cultural ingredients of writers whose supposed indebtedness to Plato and Aristotle has often inhibited appreciation of their Stoic leanings.

Reading her work as a classicist and historian of ancient philosphy, I find it much superior in volume 2 (which covers material I know less well) than in volume 1. The philosophical interest of Stoicism in its formative years has become a major subject of research during the past two decades. Colish shows an awareness of this in her bibliography (although she has missed some important items), but it is not adequately reflected in her introductory account of the school, which also contains much to mislead the novice. For instance, the Stoics did not identify God and matter (vol. 1, p. 23); they posited them as their two "principles." Colish's remarks on Posidonius (vol. 1, p. 25) do not accord with his position as attested in the references she gives. "Free act of will" (vol. 1, p. 52) has no place in orthodox Stoicism. Points such as these (I give only a sample) are the more likely to misinform because Colish writes with apparent authority; she supports her statements with copious references and evaluates highly technical scholarly works. She has not, however, fully appreciated the complexity of Stoic philosophy, nor does she sufficiently command its technicalities to make some of the judgments she offers concerning Roman writers.

She casts doubt on Stoicism as the source for a description (in Gaius's *Institutes*) of "firm and immutable" providence, on the grounds that "the Stoic order of the universe is dynamic and ever-changing" (vol. 1, p. 359). In fact, the Stoics identified providence with "destiny" and regarded both as fixed and unchanging. She correctly observes (vol. 1, p. 358) that animals do not form a moral community with people but fails to point out the Stoics' practice of founding their ethics on impulses common to people and animals. She frequently claims that the Stoics were committed to sexual equality and the unnaturalness of slavery. Actually, the small evidence on these big issues is controversial and needs to be interpreted by careful attention to its original contexts.

These and a number of other dubious assertions are deployed in an attempt to minimize the significance of Stoicism on the formulations of Roman jurists. The conclusion would be important if it were sound. Unfortunately, it is argued too casually to be accepted in the authoritative way the author represents. In particular, she is insufficiently attentive to the use of "nature" and "natural law" in the best evidence for orthodox Stoicism.

One does not expect encyclopedic works to be completely free from error. Nevertheless, a volume that consistently misspells Citium, attributes to Lucan nine books instead of ten (vol. 1, p. 254), and overlooks the *virtus* fragment of Lucilius (vol. 1, p. 160) does not fully inspire confidence. My puzzlement at volume 1 is still greater in relation to the treatment, or mistreatment, of Seneca. This writer, the most influential and prominent of all Roman Stoics, is accorded a mere seven pages in the historical introduction. Cicero, an Academic by philosophical persuasion, is given one hundred pages (some of them, I am afraid, ill-judged). In a volume

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that covers a large number of writers whose Stoic proclivities were minimal, the neglect of Seneca is incomprehensible.

In sum, Colish has produced a scholarly resource that fills some gaps. Her second volume, and some parts of her first, will be useful starting points for further study of the neglected Stoic tradition. Even so, the work as a whole, for all its great labor, falls short in penetration, interest, and control. I am sorry she did not give more thought to some of the details of Stoicism and show more discrimination and sophistication in writing about its most interesting representatives.

A. A. LONG
University of California, Berkeley

MEDIEVAL

ALLAN HARRIS CUTLER and HELEN ELMQUIST CUTLER. *The Jew as Ally of the Muslim: Medieval Roots of Anti-Semitism.* Notre Dame: University of Notre Dame Press. 1986. Pp. x, 577. $50.00.

Allan Harris Cutler and Helen Elmquist Cutler's central thesis is that "anti-Muslimism" was the primary factor in the revival of anti-Semitism during the High Middle Ages. The authors argue as follows: anti-Semitism was dormant from 700 to 1000 and might have died out completely but for the anti-Muslim feeling engendered by the Crusades; Jews had come to be identified, in the Christian mind, with Muslims and were therefore seen as allies of the Islamic enemy.

The authors do not simply repeat the sensible observation that animosity toward the Muslim infidel spilled over into animosity toward the more visible, vulnerable, and accessible Jewish infidel. They claim that the Jews were consistently seen by Latin Christendom as fifth columnists for Islam. They insist, moreover, that this perception, rather than Christian theology or economic conflict, was the primary cause of medieval anti-Semitism.

The evidence adduced by the authors seems inadequate to sustain their radical thesis. The contention that anti-Semitism was on the verge of extinction is never established. That no major persecutions are recorded in the scanty surviving sources on European Jewry from 700 to 1000 proves little. European society was still superficially Christianized, and the Jews were few and economically useful. To prove their point the authors would have to show that the church was abandoning its longstanding position on the Jews—and this they fail to do.

Moreover, the claim that Christians generally perceived Jews to be fifth columnists for Islam is never adequately documented. From a considerable corpus of medieval writing on Jews and Judaism the authors adduce not a single source that explicitly alleges an ongoing Judeo-Islamic axis and only a few that allege isolated instances of collaboration. If Jews were hated primarily as conspirators with Islam, why didn't Christians say so?

In the absence of explicit allegations, the authors have recourse to exegesis—much of it forced. For example, the authors take Gregory IX's statement that Christians should tolerate Jews in their midst just as they hope that Christians living under Islam will be tolerated to mean that "Muslims and Jews are brothers . . . if we want Muslims to treat our brothers (the Christians) . . . kindly, we must treat *their brothers* (the Jews) . . . kindly" (p. 99). They do not mention Innocent III's claim that Muslims cannot tolerate Jews and "vehemently rebuke" Christians for granting them toleration.

The Fourth Lateran Council, held under Innocent III, decreed that Jews and Muslims must be distinguishable from Christians by a difference in their clothes. According to the authors, this decree was aimed primarily at the Muslims of the Middle East, who were to be conquered, socially degraded, and then converted in preparation for the Second Coming, which Innocent expected in 1284. The inclusion of the Jews was secondary—a result of their presumed alliance with Islam. The only advantage of this convoluted theory is that it supports the authors' thesis.

The authors suggest that in several literary works, which eventually came to the attention of Christians, Jews identified themselves with Muslims and thus "unwittingly" contributed to anti-Semitism. The books in question do speak of Jews who did well in the service of Muslim princes, but they contain nothing that could be construed as indicating an alliance between Jews and Muslims and probably exerted no influence on Christian public opinion. The work that the authors consider to have been the most "pivotal" (*The Chronicle of Ahimaaz*) was probably unknown to the vast majority of medieval Jews.

The authors devote much speculation to Jewish conversion to Christianity in late medieval Spain and the consequent Marrano problem. They are convinced that "the fundamental truth of Christian-Jewish relations for the past 2,000 years" is "that the Jews desperately seek complete assimilation into the Christian world, but the Christian world just as desperately cannot afford to allow them to assimilate completely" (p. 243). They are further convinced that Spanish Christianity held a special attraction for the Jews because it was heavily "Semiticized." The book's final chapter presses the farfetched proposition that contemporary anti-Semitism is still a product of "anti-Muslimism" and that its elimination,

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therefore, primarily depends on Christian-Muslim reconciliation.

This book is obviously the result of much labor and devotion, and to have to reject its conclusions so roundly is painful. Much can be learned from some of the issues raised by the authors and especially from the rich bibliography of secondary literature that they have amassed. But the problem posed by the book still awaits an open-ended examination of the evidence and a more sober mode of historical analysis.

BERNARD SEPTIMUS
Harvard University

TORE S. NYBERG. *Die Kirche in Skandinavien: Mitteleuropäischer und englischer Einfluss im 11. und 12. Jahrhundert; Anfänge der Domkapitel Børglum und Odense in Dänemark.* (Beiträge zur Geschichte und Quellenkunde des Mittelalters, number 10.) Sigmaringen: Jan Thorbecke. 1986. Pp. 197.

When the church expanded beyond the *limes* of the Roman empire, it was faced with the problem of organizing the new territories. Originally, Northern Europe was laid out as a mission field under the archbishop of Hamburg-Bremen, but in the twelfth century three new archbishoprics emerged, corresponding to the Scandinavian monarchies. The interplay between the imported ecclesiastical organization in bishoprics, parishes, and monastic chapters and the native structure (both secular and religious) is the subject of this book. It consists of three individual parts, loosely held together by the title. The first chapter, comprising one-half of the book, deals with the large picture of the episcopal subdivisions of the three Scandinavian countries. To this well-known field Tore S. Nyberg adds new insights, particularly concerning Sweden. Of greatest interest is his analysis of the geographic perceptions found in Adam of Bremen, the *Provincialis* list (which he calls the Florence list), and in a text possibly of Joachim of Fiore treating these areas. He convincingly argues that Continental Christendom was overwhelmingly more influential on the Nordic ecclesiastical organization than were the Anglo-Saxons.

The remainder of the book contains the first systematic studies of the regular chapters at Børglum and Odense in Denmark. These monasteries became episcopal centers for large islands, Børglum in northern Jutland and Odense on Fyen. In Børglum a chapter of regular canons substituted for the lack of a *civitas*, and in Odense the cult of the murdered King Knut (1086) that was promoted by English monks made this chapter a strong ecclesiastical presence. By a careful analysis of the Scandinavian and German sources Nyberg demonstrates that the Børglum chapter, the newest of such organizations in Denmark, was founded already in 1139 by regular canons from Steinfeld and became associated with the Premonstratensians at a later date. In Odense the chapter's original dependence on the Benedictine mother cloister at Evesham declined toward the end of the twelfth century, when the monks became involved in a power struggle between King Valdemar I and Archbishop Eskil occasioned by the papal schism. Nyberg deftly leads the reader through these complicated events, but his book was completed before he could consider the important new interpretation by Carsten Breengaard, *Muren om Israels hus: Regnum og sacerdotium i Danmark 1050–1170* (1982).

JENNY M. JOCHENS
Towson State University

RUTH HILDEBRAND. *Herzog Lothar von Sachsen.* (Beiträge zur Geschichte Niedersachsens und Westfalens.) Hildesheim: August Lax. 1986. Pp. 174.

Fifty years ago Ruth Hildebrand wrote a controversial book about Henry the Lion, *Der sächsische "Staat" Heinrichs des Löwen* (1937), in which she argued that Henry was not a stem duke but the creator of a Saxon "state." She has now turned her attention to the twenty-year ducal reign (1106–25) of Henry's maternal grandfather, Emperor Lothar of Süpplingenburg. Unlike Herbert Vogt, who felt that Lothar exercised some superior ducal authority, Hildebrand argues that Lothar was a capable political and military leader whose position depended on the counties and advocacies he controlled. Hildebrand thus joins such scholars as Marie-Luise Crone, Franz-Josef Schmale, and Heinz Stoob who have altered in the last two decades the traditional picture of Lothar as a Saxon particularist and usurper. The "new" Lothar made Saxony for the last time the heartland of the German monarchy, set the course for his grandson, and was not a compliant tool of the church.

I was, however, disappointed by the book. Its purpose became clear only after I read the conclusion. A historiographical summary, genealogies, and maps would greatly help the nonspecialist. Some points require further elucidation, for example, the precise rights Lothar acquired as duke in 1106. Although Hildebrand is correct that Lothar and Henry as the heirs of the Billungs were hardly stem dukes, Henry's enforcement of the peace throughout the duchy was probably based on some residual ducal authority. The claims of Lothar's relatives to the Orlamünde inheritance may have been, as Hildebrand asserts, a neglected cause of the Saxon opposition to the Salians, but she ignores the Sax-

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ons' other political and religious grievances studied by Lutz Fenske. I am not convinced that the fourteen-year-old Lothar captured Archbishop Liemar of Bremen in a "stroke of genius" (*Geniestreich* [p. 31]) and forced Liemar to enfeoff him with the advocacy of Bremen, particularly because Hildebrand admits that Bremen lay outside Lothar's sphere of influence in 1089. And I would like to see some additional evidence that the famous "conjuratio pro libertate" in Cologne in 1112 was a conspiracy not of burghers but of Rhenish princes. Hildebrand makes some factual mistakes; for instance, Paschal II was no longer pope in 1119 (p. 59). Finally, to describe an eleventh-century count as exercising *staatliche Rechte* (p. 11) is to misconstrue the nature of medieval lordship, and to call Lothar a *Realpolitiker* (p. 108) is anachronistic. Specialists will find the book useful, but other readers will be more confused than enlightened.

JOHN B. FREED
Illinois State University

MARIO MONTORZI. *Fides in rem publicam: Ambiguità e tecniche del Diritto Comune*. (Storia e diritto, number 12.) Naples: Jovene. 1984. Pp. xxiv, 615.

Mario Montorzi attempts to analyze the instrumental role played by the medieval legal profession in legitimating the coercive authority and power of the state, which refers, I think, to the city-states and principalities dotting Italy from the fourteenth century on. Beginning in the twelfth century, the author contends, Italian jurists, notaries, chancellors, and officials charged with administering the law conspired to liberate these states from the universal tutelage of empire and papacy. They provided an integrative common discourse, replete with metaphors, syntactical patterns, and narrative strategies, whereby the state became morally justified in the eyes of its subjects and became an absolute and objective reality grounded in their faith. Such faith also resulted in the routine *ex ante* acceptance of the legality of state intervention in the lives of its subjects.

The author's view of this metaevent is negative. He believes that the state is the outcome of a refined rhetorical game that masks the wickedness of political coercion under the dress of an omnipresent and benevolent power. So conceived, *fides in rem publicam* constitutes an ideological construct crafted and manipulated by a legal profession bent on securing the loyalty of subjects, whereas the faith of subjects in the benevolence of the state turns out to be false consciousness. The medieval legal profession is indicted both for its arrogance in attempting to establish its particular doctrines, rules, and methods as an encyclopedic science in which answers to any problem could be found and for its bad faith in so unashamedly wielding this science to mystify subjects in the interests of the state. The texts marshaled to support this indictment range from fragments of medieval civil and canon law to fragments of Renaissance humanism. The author's own text includes editions of the *prooemium* to the *Summaria in Valerium Maximum* by the Neopolitan jurist Luca da Penne and an interesting oration on justice by the Florentine humanist Giannozzo Manetti.

Scholars acquainted with the research and theses of Francesco Ercole, Francesco Calasso, Walter Ullmann, Gaines Post, Ernst Kantorowicz, and Brian Tierny should not be surprised that medieval jurists made substantial contributions to the conceptual foundations of the early modern state. What might surprise them is Montorzi's blanket accusation, running across five hundred pages, that the medieval legal profession is indirectly responsible for the techniques of deception and reflexes of concealment of modern states. Montorzi's case, in my view, remains unpersuasive. It overlooks the polar difference between medieval jurists, such as Bartolus, who attacked tyranny and the deceptions of tyrants as illegitimate and the theorists of the sixteenth century who advocated political deceit and reason of state. Montorzi also neglects to mention the conflicts between jurists and the governments they served over issues such as the interpretation and enforcement of statutes, tax privileges and immunities, ecclesiastical liberties, and the relations between capital cities and their subjects. He does not consider the ways in which laws are manipulated by those presumably subject to their repressive force for their own advantage. And not least, almost all the evidence cited by Montorzi could easily be used to construct a case diametrically opposed to his—that is, to show that a self-reflective jurisprudence wedded to the idea of justice constituted a major bulwark against arbitrary and capricious rule. Ultimately, his case rests not on testable arguments but on the assumption that the power of the state is essentially repressive, with the corollary that any person or group connected with its development must be exposed for having abetted evil. The theme of public trust as both a source and consequence of legitimacy in the Italian city states awaits a more sympathetic historian.

JULIUS KIRSHNER
University of Chicago

R. W. SOUTHERN. *Robert Grosseteste: The Growth of an English Mind in Medieval Europe*. New York: Clarendon Press of Oxford University Press. 1986. Pp. xii, 337. $55.00.

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R. W. Southern has picked just the right time in the development of Grosseteste scholarship to write a book surveying the writings by and about Grosseteste and attempting to fit him into the larger arena of both England and Christendom, while making some thought-provoking suggestions on many of the fine points of recent research. Southern's most radical departure from accepted interpretations is probably his alternative view of Grosseteste's early life. As Southern presents him, Grosseteste was born in poverty and probably educated through the largess of a patron at Lincoln. Grosseteste then went to Cambridge for advanced studies. He could not afford study abroad and so sought his fortune in the provincial courts, acting as physician, astrologer, chancellor, and whatever else was required of him. He was twice frustrated in his expectations of a benefice because of the death of his patrons and eked out a precarious unbeneficed living as an independent *magister*. Around 1220 he began teaching at Oxford; in 1225, at the age of fifty-five, he received his first benefice and became a priest; and then he rapidly rose to preeminence among the Oxford *magistri*.

Southern contrasts Grosseteste's mind, work habits, and attitudes with those of Englishmen, usually of higher birth or blessed with a generous patron, who studied at Paris and there became imbued with the attitudes and methods of scholasticism. Grosseteste was notoriously unscholastic. He was throughout his life strong-willed and independent-minded, unsystematic in his writings, willing, unlike most scholastics, to suggest a possible solution to a problem rather than determine a definitive answer, and convinced of the possibility of progress in knowledge. He began his career as a scientist, gradually shifted to theology, and composed his major scientific works, especially *De luce*, as a result of his unification of science and theology into a single field of knowledge.

Preliminary to his investigations of the development of Grosseteste's thought, Southern re-examines the dates and authenticity of many works attributed to him, largely to break the logjam in the period 1230–35 and also to find some evidence of his earlier works. As the result of this re-examination, Southern removes *De generatione stellarum* and *De fluxu et refluxu maris* from the list of Grosseteste's authentic writings, moves *De cometis* back to about 1200 and both the *Commentary on the Posterior Analytics* and the earlier parts of the *Commentary on the Physics* to ca. 1220, moves *De luce* ahead to ca. 1235–40, and changes the order of the three *Computi* and assigns the earlier ones to ca. 1195–1205. (Since Southern wrote, it has been convincingly suggested that the two earlier *Computi* are not by Grosseteste at all.)

This book is a brilliant study of Grosseteste and his times. At every point it is richly suggestive. Like most works of this kind, it will not convince every reader on every point, but it will certainly require us to re-examine our views and take Southern's suggestions into account.

The only part of the book I found disappointing was the study of Grosseteste's place in history. This is, to be sure, an ambitious undertaking for one brief chapter, and, although much of the chapter is exemplary, it nevertheless repeats the traditional view that Grosseteste was ignored for about a century after his death and was only "rediscovered" about 1350. During the past thirty years we have accumulated a good deal of fragmentary information indicating the continuity of Grosseteste's influence during this period on thinkers including Richard Rufus, Richard Fishacre, an anonymous English Franciscan in Vatican MS Ottobon. lat. 185, Adam of Puteorum Villa (author of *De intelligentiis*), Bonaventure, Gilbert of Tournai, Pecham, ps.-Grosseteste, Roger Marston, and Henry of Harclay. Southern is one of the few scholars with the erudition necessary to provide a coherent account of this influence, but, for the sake (so it seems) of the shape of his chapter, he decided not to.

But this a small complaint about an important study. It will be of great profit both to Grosseteste scholars and to others, for it provides important insights not only into the life and mind of Grosseteste but also into the age in which he lived.

RICHARD C. DALES
University of Southern California

CHRIS GIVEN-WILSON. *The Royal Household and the King's Affinity: Service, Politics, and Finance in England, 1360–1413*. New Haven: Yale University Press. 1986. Pp. viii, 327.

ANTHONY TUCK. *Crown and Nobility, 1272–1461: Political Conflict in Late Medieval England*. Totowa, N.J.: Barnes and Noble. 1985. Pp. 367. $28.75.

Constitutional, legal, institutional, and military history may be dead in France, where it has been put to route by Annaliste history, and in the United States, where such old-fashioned history is virtually sneered at by the new breeds of historians who preach that the only relevant and redeeming history is that orientated to social problems. If these two books are any indication of the present state of the art in England, then the time-honored history of William Stubbs, Frederic William Maitland, and T. F. Tout, not to mention political and military history, is still riding high.

Chris Given-Wilson has written a book that is solidly anchored in archival records, published documents, and the principal secondary authorities. It

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is financial and administrative history in the tradition of Tout and provides new information and perspectives on the royal household and the multifarious personnel associated with it. The introductory chapter on the household and political developments before 1360 tells us nothing that we did not already know. Thereafter, except for the constitutional struggles and crises that impinged on the powers and functions of the household, Given-Wilson generally introduces us to matters hitherto left in the shadows. His scrupulous counting has enabled him to conclude that the permanent staff of the household between 1360 and 1413 fluctuated between four hundred and seven hundred. What explains this fluctuation? The personnel of the household expanded when the king was waging war but then contracted during periods of peace. In the late fourteenth century, however, the growing royal appetite for pomp, ceremony, luxury, and privacy apparently dictated households larger than military, administrative, and domestic needs warranted. The household thus remained bloated even after the kings ceased their frequent campaigns and increasingly remained in the London area.

Given-Wilson gives a detailed description of the household personnel and their functions, ranging from the highest, such as the keeper and controller of the wardrobe, down to the lowest of domestic servitors who labored in the scullery and who cared for the packhorses and wagons. Such information is well known but that the marshal was responsible for protecting the household from unwanted prostitutes is less known. A treatise on the duties of the chief marshal instructed him to employ twelve *damoiselles* who were prostitutes and were protecting their profession at court. It can be concluded, therefore, that something like an organized brothel was associated with the royal household. Other details, mostly gleaned from the wardrobe books, inform us about minstrels, musicians, gambling, chess, banquets, archery, tournaments, hunting parties, and purchases of luxury items such as jewels and garments. And we learn down to the lowest fraction of a penny how much it cost the king to keep his dogs, falcons, and hawks. All of this cost much more per annum than the alms he distributed. Because of the pervasive practice of purveyance, the improperly exercised jurisdiction of the court of the verge (court of the steward and marshal), the presence of favorites and aliens, and the household's association with the kings' incessant demands for money, it is no wonder that chroniclers inveighed against the household and that such enactments as the Ordinances of 1311 attempted to control the officers and their powers. Yes, even this monograph cannot escape a little social history.

After a discussion of the financial functions of the wardrobe, the great and privy wardrobes, and chamber and their relations with the exchequer, Given-Wilson investigates the remedies proposed and attempted to make these financial organs more efficient and less financially burdensome. A chapter spells out who the royal courtiers were: now and then the king's mistress as, for example, Alice Perrers, powerful magnates and councilors (who steadily lost their influence), knights of the chamber under Richard II, clerks (who formed a kind of civil service and who survived depositions and other changes in power), and an amorphous body of the king's friends or favorites. A final chapter on the king's affinity concerns those connected with the king: individuals of an exalted political and official level who constituted the inner circle, a second circle of the ordinary household functionaries without political importance, and a third and outer circle of those retained as knights, squires, archers, and yeomen. Concentrating on the outer circle, this chapter provides details on the operation of the system of indenture and maintenance and explains its military and political consequences. Contrary to recent opinion that royal retainers had little political influence in Parliament, Given-Wilson concludes, quite rightly I think, that such retainers were political operators with clout that could sway the commoners.

This is a good book, but it is not without faults. The bibliography is almost devoid of studies written by other than English scholars, a weakness that contributes to a certain insularity. The section on the indenture system ignores the feudal antecedent of this nonfeudal practice: the money fief (*fief-rente*). And how can Given-Wilson conclude that in France, for example, the local powers of the great nobles limited royal patronage, whereas in England "a vast store of patronage" was "in the hands of the king" (p. 199)? From the thirteenth century onward limitations on royal patronage grew, many of them forced on the kings by Parliament. Never did such a thing occur in medieval France.

We turn now from a book that presents extensive archival research to one that need not have been written. Nothing is wrong with writing a narrative political and military history of England between 1272 and 1461 if such a history did not already exist or if the new book presented new interpretations and conclusions. Unfortunately, this book by Anthony Tuck is not the first such history and presents no novel points of view. Most of its facts can be found in *The Oxford History of England* and in the spate of studies recently devoted to the fifteenth century. The assessments and conclusion are mostly those of Stubbs, F. M. Powicke, May McKisack, E. F. Jacob, Charles Ross, and others. No one can argue with the following evaluation of Edward I, but it has been the assessment of this king for at least a century. "At his death he left behind him not only a massive debt and an unsubdued Scotland, but also a

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nobility which had had little reason over the past ten years to trust or to co-operate with the king. None of these problems was to prove capable of solution by his son" (p. 49). Tucker calls Edward II one of the most incompetent of England's medieval kings. Nothing new here. Richard II was arrogant and petulant and "sought to base his rule on fear" (p. 222). Nothing new here. Henry VI had some mental problems. And Edward IV failed to provide for the undisputed succession of his heir. Charles Ross said exactly this in 1974 in his book on Edward IV.

This book is a readable and concise account of two hundred years of English history but was hardly necessary. Why Tuck decided to write it remains a puzzle. Perhaps he did because, as he acknowledges, G. R. Elton suggested that he do so.

BRYCE LYON
Brown University

MODERN EUROPE

WITOLD KULA. *Measures and Men*. Translated by RICHARD SZRETER. Princeton: Princeton University Press. 1986. Pp. x, 386. $39.50.

Historians of any era, region, or subject are handicapped when not conversant with prevailing systems of weights and measures. Good published works in historical metrology therefore provide a valuable tool to the profession. This is particularly true when previously obscure material is made available. The distinguished Polish economic historian Witold Kula included a chapter on metrology in his 1963 study, *Problemy i metody historii gospodarczej* (Problems and Methods of Economic History), a book known primarily to specialists; this volume represents an extension of these investigations. The publication of Richard Szreter's English translation of this study significantly adds to our knowledge of metrological systems and practices in Poland, Russia, and other areas of Eastern Europe. This work both complements such studies as Ronald Zupko's detailed compilations of British and French weights and measures and offers an interesting comparative history.

The first part of the book and a concluding chapter deal with measurement from a worldwide perspective. In this cross-cultural discussion Kula makes numerous references to such diverse subjects as the use of the sound of the bull's bellow for land measurement in Latvia as late as 1947, Ashanti methods for weighing gold, dynastic reforms of measurement in China, and the diffusion of the metric system throughout the world since the eighteenth century. Although Polish developments are highlighted throughout, a second separate section of the book exclusively deals with that nation. A key chapter on Polish attempts at standardization from the sixteenth through the twentieth centuries has, however, been reduced to a one and one-half page summary in this English edition. Although this will suit most readers, it will disappoint the specialist.

The leitmotif of this broad-ranging study is the author's ambivalence regarding the traditional systems of weights and measures that predominated in premetric Europe. Although an economic historian, Kula emphasizes the social history of measurement by exhibiting an acute sensitivity to the virtue of traditional measures derived from the human body and human labor. These units, such as the foot and the ell (derived from the elbow), Kula describes with the Polish adjective *znaczeniowy*, coined from the word for "meaning" or "significance" and for which the translator has offered the modifier "representational." These representational measures are juxtaposed with the more abstract metric system, based not on human elements but on convention. In his respect for the humanity, proportion, and cultural significance of such representational systems, Kula shares views with many Western scholars, including American historian of science A. Hunter Dupree. Yet, Kula also relates a darker side of representational systems that, under feudal domination before the French revolution, had become a vehicle for economic exploitation.

In a chapter on the measurement of grain, for example, the author compares Polish and French data to demonstrate that measurement worked against the interests of the peasant in each country during medieval and early modern times. In the payment of feudal dues or taxes in kind, tension existed between the peasant's desire to use the "striked" or level bushel and the lord's interest in employing the "heaped" or larger measure. Similar arguments are recorded over whether such measures should be poured from arm's height and "unpressed" or from shoulder height and thus be compressed by weight. These disputes were rarely won by the peasants. Thus, for Kula the state-standardized metric system, although lacking the aesthetic qualities of its representational predecessors, represented real social progress. He states that the metric system "could only become a social reality—being in the first instance an abstract, rationalized creation, seemingly of armchair scientists—once the feudal privileges had been toppled, and equality before the law triumphed" (p. 226).

Kula employs this Marxian interpretation in the third section of the book, an account of the origins and development of the metric system in France. He constructs his argument largely through a textual analysis of *cahiers de doléances* collected from several provinces on the eve of the meeting of the Estates General in 1789. Although acknowledging views that question the *cahiers* as a reliable source—their

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accuracy, the uncertainty of authorship, the existence of exaggeration—Kula maintains their importance in establishing metrological reform as a major grievance against the French seigneurial establishment.

The book is always informative, usually interesting, and sometimes exasperating. The translator has achieved clarity, but at the price of a rather plodding and sometimes stilted prose. The array of topics covered, crossing temporal and geographical barriers at a rapid pace, is at times confusing. Yet, this eclecticism, along with the author's central interpretation regarding the advent of metrification, also makes the book enjoyable to read. Despite the flaws mentioned above, metrological specialists and historians in general will benefit from the access to Kula's work that this volume provides.

AUGUST W. GIEBELHAUS
Georgia Institute of Technology

CARLOS M. N. EIRE. *War against the Idols: The Reformation of Worship from Erasmus to Calvin*. New York: Cambridge University Press. 1986. Pp. x, 325. $37.50.

For Carlos M. N. Eire the "war against the idols" was a struggle between a "religion of immanence" and a "religion of transcendence." This struggle separated not only Catholic from Protestant but also Lutheran from Reformed. Eire traces this struggle from its roots in late medieval lay devotion, through the "transcendentalism" of Erasmus, to its uncompromising expression by such reformers as Karlstadt and Zwingli, to its practical expression in iconoclastic riots in various Swiss cities, to its culmination in the theology of Calvin, and, finally, to its revolutionary expression in Calvinism and Calvinist resistance theory throughout Europe. Eire's central contention is that "John Calvin, in defending the heritage of the Reformed attitude toward idolatry, forged a new, scripturally based, theological metaphysics in which the boundaries between the spiritual and the material were more clearly drawn than ever; and that his reaffirmation of the centrality of 'spiritual' worship, with its consequent denial of compromise, provided a solid ideological foundation for much of the social and political unrest that accompanied the spread of Calvinism" (p. 3).

I am troubled by Eire's major interpretive categories. As I read his evidence, I detect a war against idolatry, both spiritual and material, more than a war against idols. Idolatry is the Reformed term of opprobrium for improper worship of any form. Eire's insistence on a dichotomy between matter and spirit, his distinction between a "religion of immanence" and a "religion of transcendence," and his descriptive categories of "transcendentalism" and "spiritualism" (not to mention "biblicism" and "primitivism") seem inadequate to the reality that he seeks to describe and categorize. The Reformed were concerned with securing "right worship," which meant bringing body and soul, external action and internal belief, into accord with each other. As Calvin insisted, the Christian should honor God not only within the heart but also with external testimony.

To be sure, Calvin and the other Reformed leaders rejected and condemned idols. But idolatry extended beyond reverence to things created by human hands. As I read Eire's chapter on iconoclasm in Switzerland and Geneva I was struck by how often the Mass, and not the "idols," was the first and primary target of the Swiss reformers and demonstrators. Why was this so? Because the Mass was an expression of the improper devotion to the material that Eire is at pains to point out? Yes. But also because an attack on the Mass was an attack on the belief that in the Mass the priest offers a sacrifice to God, who will in turn reward with grace those for whom the sacrifice is offered. It is also an attack both on works-righteousness in its sacramental form and on clerical intercession.

Eire convincingly refutes Carlo Ginzburg's contention that Nicodemism was a unified and clearly defined European "spiritualizing" movement. He also undermines Quentin Skinner's thesis that nothing about Calvinist resistance theory is distinctly Calvinist. Finally, although the war against idolatry was only one aspect of the Reformed movement, and a transitional and negative aspect at that, it was a phase of the movement in which academic theology and popular action came together. To Eire's great credit he makes this connection and its relation to revolutionary ideology and behavior clear.

MARK U. EDWARDS, JR.
Harvard University

PAOLA ZAMBELLI, editor. *"Astrologi hallucinati": Stars and the End of the World in Luther's Time*. New York: Walter de Gruyter. 1986. Pp. x, 293. DM 128.

The history of occultist beliefs and practices intersects in many ways the general history of Renaissance civilization, but nowhere more strikingly than in the astrological predictions associated with the great conjunction of aqueous signs that took place in February 1524. This astronomical event, which led to hundreds of publications interpreting its significance, including many warnings of abnormal rainfall and even a second universal flood or the end of the world, forms the general focus of the present book, which is the product of a symposium held in Berlin in 1984 under the direction of Paola Zambelli. Scholars from several European nations

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took part. By some odd quirk of the scholarly publishing world, nearly all the papers have been translated (not always very successfully) into English, although three remain in their original language (two German, one French).

This symposium publication shares both the virtues and the faults of its type. On the negative side, no unifying synthesis provides a new (or any) general interpretation of the prognostications called forth by the approach of the fateful year. The book lacks even a simple, orderly narrative of the growth of excitement caused by the approach of such a rare and (for believers in astrological influence) fearsome astral event. Also, not all the essays deal directly with the central theme of the symposium. On the other hand, this assembly of noted scholars has produced a number of valuable essays that cast new light on special aspects of the role of astrology in Renaissance culture and also on the thought and careers of a number of individuals. If there is any overarching conclusion, it is probably the editor's contention in her paper on Luca Gaurico that prophecies of the disasters associated with the conjuction played a significant part in political and religious propaganda of the early sixteenth century, thus further developing a point she has made in several earlier publications. Other papers suggest the same opinion, although it by no means appears in all. In every essay, however, the central role of the printing press in shaping sixteenth-century culture is evident.

The editor's introduction and two essays by Krzystof Pomian and John D. North set the stage for the rest by discussing in a general way the ancient, Arabic, and medieval backgrounds and sources of Renaissance astrological beliefs. More detailed essays study the astrological beliefs of Martin Luther (by Ingetraut Ludolphy), Philipp Melanchthon (by Stefano Caroti), and Georg Spalatin (by Irmgard Höss). Broader and more interpretive in character is Helga Robinson Hammerstein's "The Battle of the Booklets: Prognostic Tradition and Proclamation of the Word in Early Sixteenth-Century Germany," in which she traces the confluence of Joachite prophecy and astrological prognostication and demonstrates how (both before and after 1524) Protestant authors transformed the printed prognostication into a vehicle for religious propaganda. Hans-Joachim Köhler next adds a quantitative analysis of the *Flugschriften* associated with the great conjunction, again emphasizing the crucial role of the press in shaping—even creating—popular opinion. Next come two studies of authors of popular astrological works, Johannes Lichtenberger (by Dietrich Kurze) and Johann Virdung von Hassfurt (by Max Steinmetz). The influence of such themes in France appears in Anne-Marie Lecoq's paper on the rhetorician Jean Thénaud, a study based on an unpublished work that shows strong influence of the writings of Albertus Pighius, Agostino Nifo, and especially Lucian, and which also suggests the author's possible connection with the young Rabelais. Zambelli's essay on Luca Gaurico follows, returning to the theme of astrological prophecy as a device of political and religious polemic and propaganda. Germana Ernst concludes the volume with an essay that traces some of these astrological and prophetic themes into the late sixteenth and early seventeenth centuries, as found in the works of Tommaso Campanella and, to a lesser extent, Tycho Brahe, Johannes Kepler, and several more obscure figures of the later sixteenth century.

Several of the essays briefly discuss sixteenth-century astrologers' attempts to respond to the severe attack on astrology launched at the end of the fifteenth century by Pico della Mirandola. As noted earlier, several set forth the role of astrological prognostications in political and religious propaganda. In general, however, the book is more successful as a collection of essays on various special aspects of its subject than as a synthetic work advancing broad generalizations. Those who already know of the prominent role played in sixteenth-century culture by occult learning will value the book chiefly as a collection of individual pieces, whereas those who are unaware of the cultural significance of Renaissance occultism will gain a general impression but no comprehensive view of occultism or even of the great conjunction of 1524. Scholars' use of the volume will be facilitated by an index of names and also an index of manuscripts and rare books.

CHARLES G. NAUERT, JR.
University of Missouri, Columbia

PETER CLARK, editor. *The European Crisis of the 1590s: Essays in Comparative History*. Boston: George Allen and Unwin. 1985. Pp. xi, 324. $29.95.

One of the best reasons for putting together a book of collective scholarship is to survey current research. By describing the results of the detailed work that is usually familiar only to specialists, such a work can both enlighten and instruct. Even those familiar with one part of the field will benefit from the reportage of their colleagues. But no publisher—and few self-respecting historians—will be content with so limited a goal. A book must have a theme, and a pointed one at that: pablum such as "A Decade of Change" even our students learn to avoid. The result is a proliferation of such dramatic images as "conflict" and "crisis," which, though often strained, at least make possible the production of immensely useful (if not necessarily unified) contributions to knowledge such as this volume.

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None of the authors is able to remain within the confines of a single decade, let alone demonstrate that any field of expertise is best understood as part of a "crisis of the 1590s." Indeed, the sixteen essays—ten on specific geographic areas, and the rest on general themes such as demography—strengthen the case for regarding the far more intense turmoil of the mid-seventeenth century as the first truly all-encompassing crisis after the vast upheaval of the Reformation. Between these two seismic eruptions came more than a century of tension and difficulty (of which the 1590s were typical), but not until the second outburst can one see the resolution of major problems, and the subsequent calming down, that is the mark of a crisis. To the extent that historians need such markers to help order the past, at least for their students, multiplying the signposts is not helpful, whether they be called crises or renaissances, but the impulse is understandable; more important is the substance that lies behind the device. And by that standard, which is more significant than the attempt to establish a new crisis, this book is a most welcome addition to the literature on early modern Europe.

In some cases, such as Timothy Davies's pathbreaking chapter on the building of new villages by aristocrats in Sicily and R. B. Outhwaite's tightly argued examination of poverty in England, the chapters are based on extensive primary rsearch. Most, however, put together a comprehensive survey of secondary works and shape them into original and highly informative assessments. Among these, the strongest argument for a crisis, at least in England, appears in Peter Clark's two contributions—one of them a general introduction, and the other a detailed study that demonstrates how widespread the social disruptions of the 1590s were in England's towns and what remedies and strokes of luck averted worse problems. An indication of how far he has to stretch to sustain the overall conception, though, is a sentence on "great cities like Rome" being overwhelmed by the poor and the Papal States being "infested with bandits" (p. 7), which distinguishes the 1590s neither from adjoining decades nor from adjoining centuries. In this regard, Outhwaite is perhaps wiser when he refuses to separate the 1590s from a half-century of troubles (p. 40).

Another contradiction concerns the relationship between malnutrition and disease. A number of contributors persist in assuming that malnutrition leads to disease without further ado, despite the conclusive evidence that the links are elusive and sporadic. Indeed, David Souden provides an authoritative, lucid discussion of the subject in this volume. Why his colleagues choose to ignore him (for example, on pp. 34, 46, and 113, on the first of which pages an outdated work is cited as the last scientific word) is difficult to understand. Other lapses occur as well. One author is reduced to citing breaks in the Dutch dikes (which were not peculiar to this decade) as evidence of the special difficulties of the 1590s (p. 76); another quotes the following cluster of solecisms with approval, "one of the modalities by which allocations of economic roles and intensifications of economic disputes occurred" (p. 272). The latter is particularly surprising, for it comes in a thoughtful chapter by I. A. A. Thompson on warfare, which, aside from making an intelligent balance sheet of those who profited as well as lost from military activity (only the gains of the building and fortifications industry are slighted), also contains the most felicitous sentence in the book. I was almost persuaded that this was indeed a decade of crisis by Thompson's *aperçu*: "The 1590's began with Philip Sydney; they ended with Don Quixote" (p. 276).

Unfortunately, that is also just about the only reference to literature in the book. The arts receive no mention. Yet one cannot argue for the distinctiveness, let alone the significance, of the 1590s without mentioning that in this decade Shakespeare began writing plays, Rubens began painting, Galileo first corresponded with Kepler, and Maderno built the first baroque church in Rome. The omission merely emphasizes that this is, above all, a resource book, whose contents might more accurately (though without hope of publication) have been described as "aspects of the political, social, economic, and demographic history of England, the Dutch Republic, France, Germany, Italy, and Spain around 1600."

The collection's greatest strength is its detailed focus on the economic and demographic experiences of the period; beyond the essays already mentioned, this concentration is especially apparent in the valuable surveys of France by Philip Benedict and Mark Greengrass, of German towns by Heinz Schilling, of Italy by Nicholas Davidson and Peter Burke, and of Spain by James Casey. And the general essays reinforce this emphasis, notably Souden's, C. S. L. Davies's on popular disorder, and Brian Pullan's on state intervention, which illuminatingly draws on Venetian materials. John Elliott, gallantly summing up, does not agree with all the authors and intriguingly points the way toward the larger explosion of the mid-seventeenth century, but he gives political forces the central role that they otherwise enjoy only in the essays by Outhwaite and Pullan and in the suggestive, unusually bleak study of Dutch social policy by Leo Noordegraaf.

Taken as a whole, this book is essential reading for the early modern historian and the graduate student. It will be ransacked for ideas and information, and individual chapters will be useful for advanced undergraduate courses. That its theme is

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not sustained is a small shortcoming when set against its genuine and weighty achievements.

THEODORE K. RABB
Princeton University

ALDO DE MADDALENA and HERMANN KELLENBENZ, editors. *La repubblica internazionale del denaro tra XV e XVII secolo.* (Atti della settimana di studio, 1984; Annali dell'Istituto storico italo-germanico, number 20.) Bologna: Il Mulino. 1986. Pp. 383. L. 34,000.

For the past several years the Istituto storico italo-germanico, under the aegis of the Istituto trentino di cultura, has held a series of seminars, each devoted to a historical theme. German and Italian historians attend these, and the acts of the proceedings are subsequently published. The volume under review, edited by Aldo de Maddalena and Hermann Kellenbenz, presents the proceedings of the conference held in September 1984.

The format of this type of seminar has both strengths and weaknesses. Obviously, it enables scholars working on comparable problems to get to know each other, to exchange views, and to inform colleagues of new ideas and findings. These not unimportant advantages must be weighed against the almost inevitable drawbacks of the genre: seldom do scholars not working in close cooperation with each other produce strictly comparable work; each applies particular methods to evidence that is not exactly commensurable, to questions that arise out of different impulses and interests. Frequently, the excitement and tension generated in the course of personal exchanges are not always evident at the time of publication, for the printed form tends to bring out the episodic and occasional nature of the undertaking. For such an initiative to work well, participants must be disciplined by an organizer, who must not only generate the overarching theme of the conference but also keep the attention of all participants firmly focused on the theme that offers intellectual cohesion to the enterprise.

The volume under review reflects some of the strengths and weaknesses of the genre. Its principal strength is the idea generated by de Maddalena, who, in an opening essay of great interest, presents a brilliantly articulated, if not entirely new, historical problem. De Maddalena focuses our attention, once again, on the fascinating question presented in the book's title: the emergence, at the very moment when nation-states were coming into their own in Western Europe, of a superstate (*sovrastato*) (p. 7), a *repubblica internazionale del denaro*, comprising those who, regardless of regional origin, "courageously ventured along perilous international economic routes" (p. 9). What was the organization of this republic of money? Its membership? What methods did its citizens employ to defend their collective interests, to expedite their affairs, to diminish the risks to which their initiatives exposed their capital, to protect themselves against state-sponsored encroachments into the symbolic territory of their republic? And what policies did the leaders of the vast empires that emerged in the sixteenth century adopt toward this international, supranational republic? Readers of Fernand Braudel and of Perry Anderson's synthetic works and of R. Carande's and J. Gentil da Silva's splendid monographs will not find these questions to be new. Yet, these readers will discover in the pages of the specialized contributions constituting this volume a wealth of documentation relating to the operations of European banking during the early modern era, mostly in the Habsburg empire. In addition to de Maddalena's fine introduction, and to the recapitulation by Kellenbez, the volume consists of eight solid essays, three of which (by Enrique Otte, Giorgio Doria, and Giulio Mandich) deal with broad problems, whereas the other authors (Othmar Pickl, Bernhardd Kirchgässner, Reinhard Hillebrandt, Valentin Vázquez de Prada, and Giovanni Muto) concentrate on individual regions, with three essays on Germanophone regions of the empire, and one each devoted to Flanders and to southern Italy.

Several themes emerge from these essays: the crucial importance of the great fairs, principally those of Piacenza, as clearing points of banking exchanges; the entrepreneurial role undertaken first by southern German bankers, the Fuggers above all, who dominated international finance until the middle of the sixteenth century, then by the Genoese, who succeeded the Germans and maintained their banking supremacy in Europe for the following seventy-odd years, and by the Portuguese, who came to dominate international finance from about the 1640s; the interpenetration of political and financial considerations and the great importance of monetary policies adopted by state governments in the histories of the great banking firms that dominated the finances of the Habsburg empire. But perhaps the most interesting and fruitful point presented here is found in Doria's fascinating essay, which seeks to locate the source of the great banking success of the Genoese in the cultural and social history of Genoa's ruling class. Possibly, Doria's fine article will serve as a model for future investigations, which might seek the engine of economic development not only in the history of entrepreneurship but also in the culture and psychology (if that is the proper expression) of the entrepreneurs themselves.

ANTHONY MOLHO
Brown University

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LUCIANO GUERCI. *Nuova storia universale dei popoli e delle civiltà*. Volume 10, part 2, *Le monarchie assolute: Permanenze e mutamenti nell' Europa del Settecento*. Turin: Unione Tipografico-Editrice Torinese. 1986. Pp. xix, 720.

This comprehensive textbook on eighteenth-century Europe forms part of a series aimed at an upper-division undergraduate or beginning graduate audience. One wishes it were available in English, for it is superior to most of the texts obtainable here. The volume is well produced (apart from a few misprints) and contains a wide range of well-chosen illustrations, which enhance the written text. The layout is simple and logical. Luciano Guerci first examines the social, economic, and demographic foundations of the Old Regime, then considers political structures and intellectual life, and finally provides a survey of internal politics in the major states and their international relations from the Peace of Utrecht to the end of the War of American Independence. The scope is impressively broad; the author treats his themes on a European scale. Thus, in his view the Enlightenment extends far beyond the salons of Paris—or the court of Frederick the Great (one discerns a debt to Franco Venturi's ideas here)—and he is just as much at home discussing the intricacies of social and political structure in England, Russia, or the minor Italian and German states as in the familiar territory of France or Prussia.

The opening chapters summarize recent research on economics, demography, and social structure. The author emphasizes the inertia of a society still rooted in a backward agrarian economy, in which forces for change were only beginning to stir; if an "agricultural revolution" was apparent in England by midcentury, it was slow to make inroads on the western edge of the Continent and would not touch the east until much later. Although commerce was expanding fast and had become a central factor in provoking international rivalries, it still made little impact in the rural backlands. Land was the real source of wealth and power. Although he refuses to be drawn into what he calls the "Homeric authorship question" of whether society was still "feudal" (p. 48), the author usefully summarizes recent debates on the nobility's changing role and the relative power of the bourgeoisie (chaps. 9–10). He stresses the resilience and adaptability of the nobles and their continuing ability to absorb new elements eager for titles and prestige. We know we are on the road to 1789, but the author poses the question of what caused the French revolution only implicitly; he is careful always to respect the specificity of the historical moment, when the present was still open-ended. For him the eighteenth century is more than a prelude to 1789.

Perhaps the book's greatest strength is its discussion of the Enlightenment. The movement of ideas is set in a social and political framework that gives it an underlying direction: toward a science of man modeled on Newton, adapting his method to establish the true principles of human felicity. At midcentury, with the publication of the *Encyclopédie*, the author sees the Enlightenment entering a more militant phase, more critical of established religion, more concerned to judge by the criterion of utility. Yet by the 1770s a new current of antirationalism began to make itself felt, at the very moment when the belief in "a future period of improvement in the state of mankind" (as Richard Price put it) seemed confirmed by events in America. At times in this section one finds the authors and titles raining thick and fast, but the line of argument nevertheless remains clear, and the author effectively conveys the extraordinary richness and complexity of the intellectual life of this period. As one would expect, Guerci offers perceptive comments on the Enlightenment in Italy, so often overlooked in surveys of this sort.

The last third of the book covers political developments. A good précis of current thinking on enlightened despotism leads into an examination of the progress of reform, state by state. The author argues that the rulers—even the most "enlightened" ones—were motivated by practical concerns rather than the desire to implement the philosophes' ideas, which in turn created an equivocal relationship between the intellectuals and those in power, admirably demonstrated by Pietro Verri and the members of *Il Caffè* when they were absorbed into the Habsburg bureaucracy, or by Diderot and Catherine II. Yet, in spite of this, the author refuses to write off enlightened despotism as a failure or a sham. Progress there was, albeit limited; the sum of human felicity increased, even if only a little. The volume closes with a valuable chronological table and an excellent bibliography. Altogether, this work is an impressive historical synthesis, lucid, intelligent, and up to date.

GEOFFREY SYMCOX
University of California, Los Angeles

CURTIS CATE. *The War of the Two Emperors: The Duel between Napoleon and Alexander, Russia, 1812*. New York: Random House. 1985. Pp. xxiii, 487. $24.95.

On the evening of June 23, 1812, the first units of Napoleon's Grande Armée crossed the Niemen River near Kovno, less than seventy miles upstream from Tilsit, where France's emperor and Alexander I had declared themselves allies five years before. For the next six months a half-million

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men under Napoleon's command, the long-suffering victims of Russian weapons and Russian weather, fought their way to Moscow and back in one of the most dramatic and disastrous campaigns of modern history. Triumphantly, this multinational host stormed Vilna and Smolensk at summer's outset, assaulted entrenchments defended by more than one hundred thousand Russians at Borodino, and marched on to occupy Moscow in the fall, only to be ordered by their emperor to retrace their steps as winter came on them in Russia's ancient capital. When the shattered remnants of the Grande Armée recrossed the Niemen in mid-December, at least eight out of every ten of its soldiers remained in Russia, captured, killed, starved, or frozen to death. Some called it the "severest campaign of six months in the annals of the world." The American ambassador to Russia concluded that "there has been nothing like it in history since the days of Xerxes."

Napoleon's ill-starred invasion of Russia has provided the inspiration for some of the great works of modern culture, not the least of which have been *War and Peace* and the *1812 Overture*. On a less exalted level, thousands of participants, commentators, and historians have written about the invasion. Memoirs and diaries of the men who marched to Moscow and the men who barred their way exist in profusion. Indeed, few of the leading figures who survived the terrible battles of the Grande Armée's advance and the even more terrible trials of its retreat resisted the impulse to record their experiences, and one would be hard-pressed to find any other nineteenth-century European event for which the sources are so rich.

Despite the wealth of first-hand accounts and the extensive research carried out by Russian, French, and German scholars over the past century and three-quarters, few modern-day English-speaking historians have paid serious attention to Napoleon's campaign. Aside from *Napoleon's Invasion of Russia*, the four-decades-old English translation of the now-classic account by the Soviet historian Evgenii Tarle, the offerings in English are unimpressive; most of them depend on a very cursory study of limited sources.

Curtis Cate's book is therefore a welcome effort to improve the quality of the accounts of the 1812 campaign that are available in English. To be sure, the book focuses more on the French side, for Cate relies far more heavily on French than on Russian sources, but he nonetheless provides an intelligent and useful chronicle not only of the actions of Napoleon and his leading generals but of their Russian opponents as well. Perhaps most important for the English reader, Cate strips away some of the myth with which Tolstoy enveloped Fieldmarshal M. I. Kutuzov in *War and Peace* and presents him in a more balanced (and critical) light. Beyond that, he restores Barclay de Tolly, who, as Russia's war minister and chief commander during the first days of the invasion, bore the brunt of the popular wrath that burst forth as the Grande Armée pressed deeper into Russia, to his rightful place as one of the empire's most able defenders.

Yet, if Cate's account passes muster as a narrative of the war's major battles, it falls short of being the broad, analytical study of the events of 1812 required for a full understanding of the confrontation between Napoleonic France and *ancien-régime* Russia that Napoleon's invasion precipitated. Cate's analysis of Russian domestic affairs, so critical for understanding that nation's war effort in 1812, is cursory at best, and his discussion of the aristocratic opposition that Alexander's policies provoked on the eve of the conflict is less accurate than one might wish. Thus, we still must await a study of Napoleon's invasion that will show the true dimensions of the confrontation between France and Russia in 1812, chronicle the magnitude of Napoleon's effort, and analyze Russia's response in all its multisided complexity. Such a work will require a careful study of voluminous Russian sources. In the meantime, Cate's book is a step in the right direction, for it tells part of a complex story with clarity and attention to detail.

W. BRUCE LINCOLN
Northern Illinois University

MARGARET M. O'DWYER. *The Papacy in the Age of Napoleon and the Restoration: Pius VII, 1800–1823*. Lanham, Md.: University Press of America. 1985. Pp. x, 285. Cloth $21.50, paper $12.75.

More than twenty-five years have passed since the last study in English of Pius VII and his papacy. Margaret M. O'Dwyer's new work, therefore, is welcome. It breaks no new ground, but its special merits are threefold: it concentrates on papacy and pope rather than on the Catholic church in general; it is a balanced treatment of the subject; it is, within a relatively brief length, comprehensive.

The first part of the book describes the world in which Gregorio Chiaramonti lived his early life as monk, professor, and bishop, his career as cardinal from 1785 to 1800, and his election in 1800 at the long deadlocked conclave at Venice. As pope, Pius VII faced a new century and a new world aborning. The tasks ahead were daunting: the new pope had to return to Rome, restore papal government, and deal with the revolutionary destruction, Bonaparte and a concordat, and postrevolutionary settlements in Italy and the empire.

The second part of the narrative examines the relations between pope and caesar, the journey to

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Paris, the limits of accommodation, imprisonment, and Pius's final triumphant return to Rome in 1814.

The restoration papacy and an assessment of the pontificate make up the last third of the book. The author clearly demonstrates that, even if Pius listened at great length to both sides and stood firm on what he regarded as essentials in the church, he nonetheless usually opted for realistic accommodation. The major problems after 1815 included reorganization of the national churches, another restoration of government in papal territories, the church in newly independent Latin American countries, pressure to join the postrevolutionary reactionary alliances, the slave trade, Austria's push for hegemony in Italy, and the recurrent revolutions in 1820 and 1821. Reconstruction was complicated by the disagreements of the "zelanti" in the papal curia who advocated a rigid return to the prerevolutionary church and the moderates who recognized the need of new approaches to a new world.

Pius's understanding of that new world and of the ideas and forces that made it was limited. But, compared to those who expected counterrevolution to prevail, he and his greatest advisor, Ercole Consalvi, were pragmatic moderates. Most important, but not analyzed in detail, Pius through some of his decisions helped create the current papacy, more centralized and more powerful in the church than ever before.

The book reads clearly and directly, despite some infelicities of style. Printing from typewritten, camera-ready copy is understandable and acceptable in these days of high costs, but in this instance pages too tightly printed impede readability. The bibliography is well chosen. The judgments of the author are balanced, at times almost too balanced. But, in sum, this is a very useful addition to the history of the church in the nineteenth century.

JOHN W. PADBERG
Institute of Jesuit Sources
St. Louis, Missouri

WERNER BERG. *Wirtschaft und Gesellschaft in Deutschland und Grossbritannien im Übergang zum "organisierten Kapitalismus": Unternehmer, Angestellte, Arbeiter und Staat im Steinkohlenbergbau des Ruhrgebietes und von Südwales 1850–1914.* (Volkswirtschaftliche Schriften, number 339.) Berlin: Duncker und Humblot. 1984. Pp. 888. DM 220.

One coal mining district may look very much like any other. Werner Berg's exhaustive comparison of the Ruhr and South Wales coal fields shows that appearances can be deceiving. Although both areas became major European coal fields in the nineteenth century, they exhibited different patterns of development mirroring important contrasts in British and German industrialization. The Ruhr region experienced economic "take-off" later than South Wales, but then, from the 1850s, it attained a faster rate of growth. Ruhr employers were committed to the use of modern technology, to capital concentration, and to controlling the coal market by collective agreements. Welsh coal owners appeared a good deal more "individualistic" and "backward" in their managerial techniques. Berg provides convincing explanations for these different patterns of development, but he does not discuss the questions that preoccupy economic historians: was the British pattern a symptom of industrial "retardation" and a harbinger of twentieth-century industrial decline? Or was it a rational response to national and international market circumstances? Could the Ruhr coal industry have served as a model for South Wales, or in economic, as in political history, is each national case unique?

The most original aspect of Berg's study is his attempt to describe how each of the social groups involved in industrial production—not just manual workers, but employers and white-collar employees as well—became aware of and began to voice their interests. Unfortunately, Berg's analysis of the formation of "group consciousness" among the people involved in the mining industries of the Ruhr and South Wales does not pay enough attention to the distinctive political histories of these two regions. As early as the 1870s Ruhr miners' support for political Catholicism and, later, for Christian trade unionism reflected the importance of the Catholic religion in this region and the miners' disaffection from their employers' National Liberal political and economic ideas. By the 1890s another form of oppositional "labor politics," social democracy, had also begun to make inroads in the Ruhr electorate. In South Wales, on the other hand, "the Liberal electoral hegemony remained largely undisturbed . . . through to 1914" and the Labour and Independent Labour parties found this region very difficult to penetrate (David Howell, *British Workers and the Independent Labour Party, 1888–1906* [pp. 27, 243]). What were the causes and consequences of these contrasting regional political cultures? Berg provides no explanations.

Berg's comparative approach permits us to see more clearly the different roles played by the British and the German states in the process of industrialization. The basic outlines are familiar. The Prussian state played a direct, active role in the mining industry, even after the liberal reforms of the 1860s. The British state, on the other hand, was largely content to allow market forces and private interests to shape the development of the South Wales coal industry. In Germany the state normally took the employers' side in labor disputes, but in South Wales the British state attempted to conciliate conflicting

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interests. Berg attempts to explain these differences in terms of the "preindustrial traditions" of the German and British states. The preindustrial Prussian state had a powerful bureaucracy with which it attempted to transform German society. This interventionist tradition continued to guide the activities of the Prusso-German state in the industrial era. The preindustrial traditions of the British state, on the other hand, disposed it toward a "laissez-faire" posture in the nineteenth century.

This is not a convincing interpretation. The evolution of modern states in Prussia-Germany and Britain was more deeply affected by the emerging class conflicts of industrial society than by "preindustrial" traditions. The ideas, strategies, and choices of nineteenth-century political actors, not disembodied "preindustrial" state traditions, produced the different relationships between the working class and the state in Germany and Britain. Otto von Bismarck and William II responded to working-class grievances by stubbornly excluding workers and Social Democrats from political influence, but a British prime minister such as William Gladstone saw the integration of the working class into the Liberal movement as a way of achieving political stability. Unfortunately, Berg's discussion of the relationship between state and society does not attempt to explain the political integration of the working class in late nineteenth-century Britain or the political isolation of workers in Wilhelmine Germany.

DAVID F. CREW
University of Texas

NATHAN WEINSTOCK. *Le pain de misère: Histoire du mouvement ouvrier juif en Europe*. Volume 1, *L'empire russe jusq'en 1914*; volume 2, *L'Europe centrale et occidentale jusq'en 1914*. (Textes à l'Appui.) Paris: La Découverte. 1984. Pp. 311; 155. 200 fr. the set.

NATHAN WEINSTOCK. *Le pain de misère: Histoire du mouvement ouvrier juif en Europe*. Volume 3, *L'Europe centrale et occidentale, 1914–1945*. (Textes à l'Appui.) Paris: La Découverte. 1986. Pp. 274. 130 fr.

Early in this three-volume work the author cites a conversation between a doctoral student and a well-known professor at the University of Geneva. The student proposes to write a dissertation on the Jewish proletariat, and the professor asks, "But are there any Jewish workers? I thought all Jews were bankers."

Nathan Weinstock not only demonstrates in careful detail that there indeed was a Jewish working class but also provides a careful account of its geographical distribution, its development, and its areas of specialization, including branches of industry from which Jews were excluded. He informs the reader about the changing class structure of the Jewish community in tsarist Russia and elsewhere, the many issues that divided it, the goals, tactics, and leading personalities of the organizations it created, and its ultimate destruction. This work quite naturally emphasizes the autonomous Jewish organization within the Russian Marxist movement, the *Bund*. But Weinstock also provides chapters on anarchist, Zionist, Territorialist, and other organizations; and, whereas the *Bund* originated in northern Poland, Lithuania, and Belorussia, he also deals with Jewish workers' organizations in Bessarabia, Galicia, Western Europe, and Thessaloniki.

The work is based not on original research but on extensive reading of memoirs and monographs in Yiddish, French, German, and, occasionally, English. Notably absent are references to sources in Russian and Polish. Yet the work is filled with information that is likely to be new to most readers, including specialists in Jewish history and in labor history. Hence, the work is a welcome addition to the literature.

Weinstock writes about the *Bund* with undisguised admiration. He brings out the heroism of its militants, the wealth of innovative and effective tactics it pioneered, and its considerable influence within Russian and Polish Marxism. He embeds the story of the *Bund* in a summary sociology of the entire Jewish community in Russian Poland and, furthermore, provides a comprehensive account of the many painful dilemmas facing not only the *Bund* but all other Jewish workers' organizations. He seems to agree with Trotsky's unkind judgment that the *Bund*, believing it represented socialism to the Jews, actually became the representative of Jewish interests vis-à-vis socialism.

Nonetheless, the *Bund* is the hero of the work. Its heroic efforts are contrasted with the cowardice of other organizations, its temporary importance with the comparative feebleness of its rivals. Similarly, the courage of the working-class Jews in Eastern Europe is presented against the background of cowardly behavior attributed to the Jewish bourgeoisie and to the Jews of Central and Western Europe. Weinstock heaps scorn on all those who did not foresee the Final Solution. He is bitter also about the Zionist reaction to Hitler; he accuses them of using the threat of extermination for narrow partisan purposes.

His admiration for the Jewish workers' movement notwithstanding, Weinstock brings out its structural weaknesses and insuperable dilemmas; in the end he argues that even without Hitler and Stalin the separate existence of a Jewish proletarian organization would have come to a close, so that the *Bund* by necessity remained a transitory phenomenon. But to this phenomenon he has erected a worthy monument.

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There are minor flaws. Weinstock's unfamiliarity with Slavic languages has led him to render some names of cities in rather odd fashion. Also, *pace* Weinstock, Yiddish is not closely related to *Plattdeutsch*.

ALFRED G. MEYER
University of Michigan

WOLFGANG STEGLICH, editor. *Die Friedensversuche der kriegsführenden Mächte im Sommer und Herbst 1917: Quellenkritische Untersuchungen, Akten und Vernehmungsprotokolle*. (Quellen und Studien zu den Friedensversuchen des Ersten Weltkrieges, number 4.) Stuttgart: Franz Steiner. 1984. Pp. cxxviii, 722. DM 168.

WILHELM ERNST WINTERHAGER. *Mission für den Frieden: Europäische Mächtepolitik und dänische Friedensvermittlung im Ersten Weltkrieg; Vom August 1914 bis zum italienischen Kriegseintritt Mai 1915*. (Quellen und Studien zu den Friedensversuchen des Ersten Weltkrieges, number 5.) Stuttgart: Franz Steiner. 1984. Pp. xii, 730. DM 139.

Herbert Asquith once described talk of a negotiated peace during World War I as "the twittering of a sparrow in a tumult that shakes the world." Wolfgang Steglich has spent the better part of his professional life attempting to show that Asquith was wrong, that at least some of the many overtures and peace feelers that emerged during the war offered realistic alternatives to a military solution of the conflict. Beginning in the late 1950s, Steglich has argued, in a series of massive publications, that the peace feelers that came at various times from the camp of the Central Powers were motivated by a sincere commitment to a compromise peace. This thesis made Steglich's work serviceable in the controversy that Gerhard Ritter, Steglich's mentor, was at the time waging with Fritz Fischer over German war aims. The two volumes under review here are part of a series that Steglich is editing on peace overtures during the war and that comprises, in addition to these volumes, Steglich's studies of the "peace policy" of the Central Powers during the last two years of the war and collections of documents pertaining to the papal mediation efforts in 1917.

Wilhelm Ernst Winterhager's book, on the tentative overtures that were exchanged through Danish mediation between London and Berlin during the first nine months of the war, shows many signs of Steglich's influence—in the thoroughness of its research, its exhaustive attention to detail, and its determination to show the perversity of Fischer's argument that the German leadership, especially Theobald von Bethmann Hollweg, was playing a clever diplomatic game with the goal of splitting the entente and winning far-flung German annexations in Eastern and Western Europe. If German leaders briefly entertained the thought of major annexations during the summer of 1914, the author argues, the military setbacks of the fall quickly disabused them of their illusions and convinced them to subordinate their designs to securing a just peace. "The readiness of the leadership in Berlin to react to this new situation with a course of steadfastly seeking peace," writes Winterhager in conclusion, "speaks for the extent to which rationality and feelings of responsibility remained alive in German policy even during the war" (p. 658). Unfortunately, the author contends, these honorable intentions were not reciprocated in the enemy camp. Although Edward Grey shared Bethmann's desire for a peace of accommodation and struggled to find a solution to the Belgian problem that the Germans could accept, Grey's allies, the French and Russians, embraced an ambitious program of war aims that admitted of no accommodation and made British compromise with the Germans impossible.

Winterhager's study invites a number of observations. Even those who have difficulty accepting the hostile portrayal of the German chancellor in Fischer's work will not be convinced by Winterhager's picture of a man whose ideal was "the great idea of humanity" (p. 541) and whose principal aim was to construct a "Western cultural block" out of Germany, England, and France to counteract the Russian threat to Europe and maintain world peace. This book is discouraging, however, not because Winterhager takes issue with Fischer's interpretation of Bethmann but because it could have been written before the Fischer controversy ever took place. The narrative is played out in the narrow corridors of power, in the discussions among leading ministers in Berlin and London, with soldiers and influential private citizens making occasional appearances. The kinds of domestic constraints that operated on the statesmen's perceptions of diplomatic events and to which Fischer began to draw attention in the case of Germany—interest groups and patriotic organizations, to say nothing of the cultural ambience of war—play practically no role in Winterhager's account of events in either Berlin or London. The parochialism of this kind of "traditional" diplomatic history poses many problems, which need not be rehearsed. Whether the peace feelers of the war had the remotest chance of success is impossible to judge from the narrow perspective offered here.

Steglich's new volume is more modest in its intent, although it suffers from the same limitations. He has collected and meticulously edited hundreds of documents pertaining to several peripheral "peace actions" in 1917, which took place independently of the papal mediation. Many of the sparrows who figure in the documents are shadowy figures with

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connections to intelligence services, minor royal houses, the diplomatic corps of neutral states, or the high echelons of the Catholic hierarchy. They expend their energies in attempts to bring representatives of the conflicting powers together, but little in Steglich's volume suggests that their twittering had any significant resonance.

ROGER CHICKERING
University of Oregon

A. W. B. SIMPSON. *A History of the Land Law*. 2d ed. New York: Clarendon Press of Oxford University Press. 1986. Pp. 316. Cloth $47.00, paper $19.95.

Since this book was first published in 1961, it has become the standard introduction to the history of the English land law. From the beginning A. W. B. Simpson hoped it would prove useful not only to students reading for law degrees but also to "pure historians." The second edition comes at an appropriate time, for pure historians have developed as never before an interest in the social history of law and in the history of legal ideas and institutions.

In the revised edition as in the original the author has attempted, as he puts it, "to pursue the same modest aim: to write a short simple book which will introduce its readers to the history of a very complicated subject." For social and economic historians now taking interest in the law this is both encouragement and warning. They should not expect this book to be readily comprehensible. "Simple" here can only mean "as simple as possible." Historians without legal training will have to work at the book. That said, there is no better nor more readable book from which they may gain an understanding of basic doctrinal matters. Chapter by chapter Simpson leads the reader through medieval tenures, the development of the doctrine of estates, uses and the statute of uses, perpetuities and the strict settlement, and commercial interests in land. The footnotes are purposefully designed to indicate where the reader may turn for more exhaustive treatment of particular subjects.

Two principal changes mark the new edition. A revised discussion of the strict settlement points to the continuing controversies over the social and economic effects of that device. Second, the last chapter on nineteenth-century reform has been much enlarged. Simpson emphasizes the conservative spirit of that reform and notes the "extraordinary measure of historical continuity" in the land law at least to 1925. An explanation is found in the fact that conveyancers alone were in a position to make concrete proposals for reform. As the author wryly puts it "the concept of a radical lawyer is scarcely intelligible" (p. 283).

The new edition takes full account of recent research, often in the footnotes. Compressed in one and one-half pages of text, though, is a most helpful account of S. F. C. Milsom's challenging thesis on feudalism, published in recent years. I gratefully noted also an improved system of subheadings in the new edition, which better breaks up topics into their constituent parts and highlights important issues. This relatively minor change much aids the ordinary historian in following the argument.

One can only sum up: this is the basic book for social or economic historians who have a serious interest in any aspect of English land law.

EILEEN SPRING
Baltimore, Maryland

PAUL SLACK. *The Impact of Plague in Tudor and Stuart England*. Boston: Routledge and Kegan Paul. 1985. Pp. xvi, 443. $39.95.

Paul Slack's book combines demographic, social, intellectual, and political history in a remarkably successful attempt at the "total history" of a subject. Clearly written, despite the difficulties in cutting a clear path through a thicket of sources, the book demonstrates argumentative vigor as well as mastery of manuscript documents from local and government archives, of contemporary printed sources, and of the pertinent secondary material. Never exaggerating the importance of his topic, Slack nevertheless treats responses to plague as examples of social and cultural resources that shed light on the interconnections between a variety of fields usually treated separately by historians.

Slack's analytical contributions bear on the demography, society, ideas, and politics of early modern England. His ideas on demography lend support to those who ascribe falling rates of mortality not to economic growth or crop diversification but to the provision of public goods developed by the administratively centralizing governments of the period. Using documents from the counties of Devon and Essex and from the cities of Exeter, Bristol, Norwich, and London, he carefully distinguishes instances of plague from other epidemic outbreaks, and, like others working within the French- and English-speaking historical communities, he describes the epidemiology of plague through the rat–rat-flea–human chain (whereas German historians have placed more stress on human to human transmission, which would only lend weight to Slack's arguments). Although the demographic consequences of epidemic plague were important, Slack argues, plague declined in demographic significance in the sixteenth and seventeenth centuries. More clearly, plague caused great suffering and mortality: he conservatively estimates

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the number of plague deaths in England at eight hundred twenty-two thousand from 1470 to 1670, with at least an equal number of people suffering but surviving. But an even greater significance of plague lies in its illustration of other issues, particularly the development of early modern government. Slack's argument about the final decline of epidemic plague in Europe emphasizes multiple causation and historical contingency yet suggests that perhaps the single most important cause was the plague policies developed and sometimes enforced by various English and European governments.

The heart of the book therefore concentrates on the idea that the development of Charles I's "enlightened absolutism" derived from real social needs that could be met only by national government; plague therefore inflicted unpopular royal policies on local communities. Clearly discussing the intellectual background to this development, Slack shows the great variety of ideas about plague yet argues that "secular versus religious" debates tended toward a "compromise" weighted toward the secular view that plague was a contagion spread by infected people and that public authority could prevent the spread of the infection and ameliorate its consequences. He also clearly shows that, after the mid-sixteenth century, the social and geographical concentration of plague epidemics among the poor and in the relatively unpoliced suburbs gave rise to an association of plague with poverty and filth, a "necessary condition" for action by magistrates. But the immediate source of action came from an increasingly interventionist monarchy: plague policy in England was developed by the crown and forced on reluctant local governments. From 1518 to 1578 ideas about controlling plague began to be broached by the crown but were not effective; from 1578 to 1625 government-sponsored proclamations, acts, and statutes began to develop a set of coherent social policies aimed at controlling plague; and from 1630 to 1665, especially during Charles I's personal rule, the monarchy actively strove to enforce obedience to its direction. The monarchy developed the unpopular policy of locking up together all members of an infected household. As always in the Tudor-Stuart period, problems of finance bedeviled local and national efforts to control epidemics of the pestilence and its social and economic consequences. But, in the end, police rather than finance divided people from magistrates and men of authority from one another.

Slack's book never loses sight of either large historical trends and abstract causes or of the individuals whose lives are ultimately the sources of history. In his vision social conditions establish the framework of events, but intellectual and political debate affect the direction and speed of change. As a result, the book possesses an interpretive interest that will be suggestive to virtually all historians of early modern England.

HAROLD J. COOK
University of Wisconsin

DAVID UNDERDOWN. *Revel, Riot, and Rebellion: Popular Politics and Culture in England, 1603–1660*. New York: Clarendon Press of Oxford University Press. 1985. Pp. xii, 324. $29.95.

This interesting book offers much fascinating evidence about English popular culture and politics in the first half of the seventeenth century, particularly in Dorset, Somerset, and Wiltshire. Its main argument is, however, rather schematic and does not do full justice to the rich, occasionally refractory evidence presented.

David Underdown argues that England was a land of two conflicting cultures and that this explains regional contrasts in popular political behavior and patterns of allegiance in the Civil War. The argument is rooted in the dichotomy between forest and field that is familiar from the work of Joan Thirsk and Alan Everitt. On this geographical contrast Underdown builds an argument about cultural and political divisions. The relative freedom of wood-pasture regions allowed market forces to make inroads and helped create a new middle sort elite of small gentry, clothiers, master craftsmen, and yeomen, who were Puritan supporters of Parliament and who opposed the values of the traditional social order. In these districts the middle sort attempted to impose hard work, sobriety, and regular church attendance on the unregenerate multitude. This effort to reform the manners of the alehouse-haunting poor brought Puritans into conflict with the guardians of traditional culture, including crown and bishops.

The older culture continued to thrive in regions dominated by the Anglican Church and the gentry: arable-fielden areas of nucleated villages and open fields, which in Underdown's view had not yet been deeply affected by market forces. In this premarket rural England of hierarchy and reciprocal obligations people from all social levels participated in shared festive rituals and recreations. Yeomen, husbandmen, and laborers followed the lead of their social betters in religion and politics; commitments to hierarchy and order were expressed in loyalty to church and king.

At times Underdown indicates that the realities of social and political life were more complex than the stark contrast between two cultures for which he argues. He admits the existence of wood-pasture districts, in southeast Somerset for example, whose inhabitants did not support Parliament in the Civil War. In fact, the evidence presents too many excep-

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tions, acknowledged and unacknowledged, for the argument to be entirely persuasive. The narrative of the antienclosure riots at Gillingham forest, Dorset, in the period 1643–49 (pp. 161–62) would have been more pointed if Underdown had dealt with the fact that the main object of popular ire, the earl of Elgin, was a parliamentary peer whose influence with Parliament enabled him to get military and legal protection for his property rights. The success of royalist recruiters in the Gillingham area that Underdown tentatively ascribes to poverty (p. 198) could also be explained by the unpopularity of the earl and his parliamentary backers.

Underdown's argument about cultural differences encounters some problems that are not entirely resolved. Sometimes, he notes that the reformation of manners was more than simply a Puritan campaign. On other occasions he sees it as inspired by Puritans and naturally opposed by the Anglican defenders of hierarchy. Ultimately, he appears unwilling to accept that Anglicans and Puritans may have agreed on the need for a reformation of manners. Regulation of alehouses, enforcement of poor laws, and efforts to instill sobriety and discipline in the populace stemmed as much from a governmental concern for order and stability as from a Puritan concern for moral regeneration. The enforcers of reformation acted in their official capacities as justices of the peace, grandjurymen, constables, and the like. The proposed reforms were contained in the statutes, proclamations, and conciliar instructions that local officials were charged to enforce. In fact, many of James I's pronouncements were virtually indistinguishable from those of Puritans.

Underdown argues that, in the reign of Charles I, the upholders of monarchy and the Laudian church waged a cultural counteroffensive against the moral reformers and on behalf of drink and festivity because they made "people more content with their lot and with the existing order" (p. 63). At least two phenomena make this assertion difficult to accept fully. One is the crown's insistence throughout the 1630s on the enforcement of the Book of Orders of 1631, an official blueprint for a reformation of manners. The other is the evidence presented by Kevin Sharpe in his contribution to *History and Imagination* that during the 1630s Archbishop Laud enforced a reform of manners at Oxford University.

BUCHANAN SHARP
University of California, Santa Cruz

RICHARD L. GREAVES. *Deliver Us from Evil: The Radical Underground in Britain, 1660–1663*. New York: Oxford University Press. 1986. Pp. x, 291. $29.95.

This is the first of three volumes in which Richard L. Greaves plans to trace the history of the "radical underground" between the return of Charles II and the revolution of 1688. A chapter on Irish plotting and occasional references to Scotland justify the use of "Britain" in the title, but the book chiefly deals with events in England. Historians have usually stressed the passive acceptance of the Restoration by former parliamentarians and have dismissed the plotting of disgruntled republicans, Cromwellians, and religious zealots as the work of an inconsiderable lunatic fringe. Only Wilbur C. Abbott, in a two-part article in this journal (1908–09), has treated the conspirators at length. Greaves attempts to set the record straight by examining their efforts in much greater detail.

The Restoration, Greaves confirms, was not the end of radical politics. Thomas Venner's crazy revolt in London in 1661 and the feeble northern rising in 1663 were only the tips of an iceberg of conspiracy against Charles II. Two nations inhabited Restoration England. One, by far the larger, welcomed or at least acquiesced in the return of monarchy, episcopacy, and social hierarchy. But another, smaller one refused to conform to the reestablished church in spite of persecution, and a few of its members still dreamed that the disaster of 1660 might be reversed. Most Nonconformists lived quietly, but the handful who resisted justify the attention that Greaves gives them.

The problem, both for Charles II's government and for Greaves, lies in distinguishing between the peaceful moderates and the real conspirators. Surveillance of "fanatics" became a growth industry, and plenty of informers had stories to tell. Greaves is sensibly skeptical of the reliability of many of his sources, although the line between rumor and fact is not always clear and former parliamentary moderates are sometimes lumped into the same radical category as Commonwealthmen. The government, of course, had an easy solution: if informers implicated a few pacific Nonconformists and made the whole tribe guilty by association, so much the better. As Greaves shows, exaggerated reports of plots were used against the Presbyterians in much the same way as John Pym had used alleged Catholic conspiracies against the court in 1641.

Greaves's narrative of these dark and mysterious doings is not easy reading. We are sometimes overwhelmed by torrents of names, often of obscure people accused of conspiracy on the slenderest evidence. Still, he does well to remind us both of the vitality of conventional dissent and of the continuing resistance of the more extreme millenarians and republicans. They were a small minority, and they often built absurdly optimistic castles in the air. But

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a hard core of reality lay beneath the fantasies of the informers. The "Good Old Cause" lived on.

DAVID UNDERDOWN
Yale University

FRANK T. MELTON. *Sir Robert Clayton and the Origins of English Deposit Banking, 1658–1685*. New York: Cambridge University Press. 1986. Pp. xii, 259. $49.50.

Robert Abbott and his nephew Robert Clayton were scriveners, at a time when their copying skills were no longer in fashion. Gradually, they moved into a new business, banking. For the clerks this meant an end to the old idea of apprenticeship followed by the establishment of independent households. Clayton's clerks were professionals, men who would always remain clerks. But for Lady Clayton, who lived over the bank, not much was new. Her husband's partner John Morris lived and died in the house, leaving her some jewels to pay her for tending him in his last illness. The clerks also lived in the house but could not afford jewelry. The household, like the bank, was a mixture, part medieval and part modern.

Abbott had a number of Royalist clients. He expected the business to be terminated after his death, but Clayton and Morris maintained it with the aid of the widow Abbott. Their clients included Lord Fairfax and the second Duke of Buckingham, who were doing business with George Monck at the end of the Protectorate. So was the City of London, which was depositing presents to Monck's account considerably before he began his march into England. At the Restoration the firm was rewarded with a number of lucrative governmental contracts. The core of the business, though, was mortgage lending on gentry estates. Clayton did much better than most of his competitors in guessing what an estate could produce. Often he used the mortgage in fee, under which his agents would occupy and manage an estate until the debt was paid off. Although revenues from land were falling consistently, Clayton did well for his clients.

He also did well for himself. Gentlemen deposited their money with him at their peril, for he had two sets of books. Unwary depositors might not get any interest or might have to wait for months before their money was placed. Meanwhile, Clayton and Morris would take it and put it out on their own account in short-term loans. Frank T. Melton argues that the partners did not milk the duke of Buckingham's estate, as contemporaries claimed. When they took it over they had to proceed slowly, for only dishonest stewards knew what the estate consisted of, but over time Clayton and Morris removed the embezzlers and brought both order and prosperity into the duke's affairs. The estate was finally broken up because of lack of heirs, not because of poor management by Clayton and Morris.

The Clayton papers, although they only extend to 1685 and not to Robert's death in 1707, are the largest extant Stuart private banking archive. Unfortunately, they have been dispersed. Melton has pulled most of them together and given a convincing account of them. Scrivener banking did not have a future: that lay with the goldsmith banks. But in the seventeenth century it had a real place. Scholars will find this book useful.

STEPHEN B. BAXTER
University of North Carolina

HAROLD J. COOK. *The Decline of the Old Medical Regime in Stuart London*. Ithaca: Cornell University Press. 1986. Pp. 310. $29.95.

This book is not the first to describe the failure of the College of Physicians to regulate and supervise medical practice in seventeenth-century London, but it is the first fully to explain how and why that failure occurred. Like George Clark, the college's most recent historian, Harold J. Cook draws much of his material from the college's rich archives, but he analyzes the quantity and quality of its regulatory activity in much greater detail. He is also better acquainted with the historical context within which the physicians operated. He has read most of the relevant pamphlet literature, and he has a firm understanding of the character of competing sources of medical aid. The result is a book of wide interest: an important contribution to medical history that also illuminates the political and intellectual history of seventeenth-century England.

Cook stresses first the economic background. As London grew so did both the demand for and the supply of medical treatment in what became, in effect, a buyer's market. The learned advice on health that the qualified physician was trained to provide faced damaging competition from the immediate therapy supplied by other practitioners. The apothecaries, in particular, were always the chief rivals of the college, and their final triumph in 1704 forms the conclusion to this book. Intellectual developments were equally important. Cook shows how the rise of different kinds of "science" challenged Galenism, and he gives a full account of the attacks mounted by chemical physicians that pushed the college onto the defensive for more than a decade after 1660.

Above all, the college's failure was political. Its position in the 1630s depended almost entirely on the support of the crown, striving to exercise authority in this as in other respects through corporations. The fellows trimmed their sails to the prevail-

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ing political winds in the 1640s, but lack of royal support meant that they did not dare to attempt regulatory activity between 1658 and 1680. At the end of the century a temporary revival of prosecutions and fines under James II was brought to an end by the new climate of the 1690s, when the college could no more maintain its pretensions to monopoly than could the Church of England or the Stationers' Company. Throughout the seventeenth century, in fact, the history of the college mirrored that of other regulatory bodies.

Cook illuminates several other subjects in the course of his work. The complaints and prosecutions coming before the college tell us much about the nature of medical practice; the pamphlet literature records part of the intellectual background that shaped it. Cook includes some useful reflections on the status of physicians and the learned professions in general. But this is essentially, and rightly, a political and institutional study, which has been carefully designed and finely executed.

PAUL SLACK
Exeter College
University of Oxford

R. S. NEALE. *Writing Marxist History: British Society, Economy, and Culture since 1700*. New York: Basil Blackwell. 1985. Pp. xxii, 319. $34.95.

R. S. Neale has assembled eleven of his recent articles, most of them now published for the first time. The topics covered reflect the author's broad interests: the first duke of Chandos, Samuel Clarke, Adam Smith, the Australian city of Hillgrove, and the historiography of urban history. Marxist theory is the subject of three essays, including a study of the bourgeoisie and a critique of Raymond Williams. In an unusual way the author has made a sustained effort to link together the separate items through "various devices, from mode of production to autobiography," as well as an extended metaphor from *Tristram Shandy*.

The autobiographical theme lends a special charm to the book. The introduction begins with Neale's first publication, a 1953 obituary of his grandfather, a London cabinetmaker. Subsequent chapters move "from England in 1700, ruled by agrarian capitalists like the Duke of Chandos, to Charlie Neale in Camden Town in 1900, from thence to Southall, to get myself born" (p. xxii). That event occurs in an admirable concluding chapter, "PO/X 122993," the author's serial number when he joined the British army at the age of seventeen in 1944. This is an excellent piece of working-class family history, including a vivid account of his mother's encounter with the Cheltenham Board of Guardians when she was seven.

As the title indicates, a Marxist motif runs through the book. Neale contends that the concept of mode of production, shorn of the technological determinism of vulgar Marxism, is a useful tool for the analysis of the basic components of economic systems. Oddly enough, however, in the nontheoretical articles Marxism provides little more than an occasional interpretive gloss. Thus, the thesis that capitalist relations of production created Hillgrove in Britain's image is appended as an afterthought to a detailed social, economic, and demographic study. The implausible notion that Clarke's Boyle lectures of 1704 "were to be an important, perhaps a crucial factor, in protecting the property and interests of the ruling oligarchy of agrarian capitalists and its attendant monied interest" is put forth without supporting argument (p. 23). In the theoretical studies, on the other hand, Neale tends to be prolix and digressive, as, for example, in belaboring the point that cultural materialism does not give enough weight to class relations.

In the course of the book Neale makes a number of playful references to a passage from Laurence Sterne that he quotes at some length in the introduction. Sterne compares the historian to a muleteer who tries to drive his mule in a straight line from Rome to Loretto, but, being a man of spirit, makes fifty deviations along the way. In that spirit Neale has pursued a zigzag route not only from one topic to another but also within individual articles. If an idea occurs to him, he pauses to take a look at it. A case in point is "Zapp Zapped: Property and Alienation in *Mansfield Park*." Identifying the subtext of this collection is not difficult: the author has had a lot of fun doing history.

TRYGVE R. THOLFSEN
Teachers College
Columbia University

L. L. BONGIE. *The Love of a Prince: Bonnie Prince Charlie in France, 1744–1748*. Vancouver: University of British Columbia Press. 1986. Pp. 412. $20.95.

Scholars working through the six-hundred-odd volumes of Stuart papers in the Royal Archives in Windsor Castle have sometimes neglected the boxes of mostly undated documents, particularly a box of love letters sent to Prince Charles Edward Stuart. L. L. Bongie has made good use of these to tell, for the first time, the story of the prince's first love affair with his cousin Louise de la Tour d'Auvergne, daughter of the Duc de Bouillon great chamberlain of France and wife of Jules de Rohan, Duc de Montbazon. The affair took place after the Forty-Five and was a highly passionate relationship, which lasted only six months and produced a son

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who died on January 18, 1749. Compared to Charles XII of Sweden, the prince was then in Voltaire's words the "hero of Europe," an object of adulation at the court of Versailles, where everyone wanted to know the details of his campaign in Scotland and England and his escape after many hardships through the heather. As befits a distinguished Voltairean scholar, Bongie brings out the links between Voltaire and Charles Edward. The tale told here involves several ironies, for instance the secret meetings between the lovers were known to Louise's ever-watchful mother-in-law, the Princesse de Guéméné, who wanted the prince for herself. They also aroused the attention of the Paris police, worried lest Hanoverian agents might try to assassinate him. Louise's husband never knew, and her father ended the affair. The prince behaved callously afterward, immediately taking another mistress, the Princesse de Talmont, and looking for a Protestant bride for dynastic reasons.

Working through the Rohan-Bouillon papers in the Archives nationales in Paris, Bongie has plotted the journeys to Poland of Louise's mother, Marie-Charlotte Sobieska (sister to Maria Clementina, "James III's queen"), to safeguard the vast estates and fabulous jewels of her father, Prince James Sobieski, for the Duc de Bouillon, her husband, and their children. That at one time Charles Edward wished to renounce his family's claim to the Sobieski inheritance in favor of Louise is a measure of his infatuation.

The book virtually ends with the prince's arrest at the Paris Opera and expulsion from France after the peace of Aix-la-Chapelle in 1748. He was popular with the common people of Paris, and twelve hundred troops were sent to quell the riots that followed this arrest. The Marquis d'Argenson, the French foreign minister during the Forty-Five, was scandalized at apprehending "the legitimate heir presumptive" to the crown of England like a common criminal and thought it made the French no better than Oliver Cromwell.

Bongie's book is too long and would have benefited from judicious cuts and a better synthesis of the material. Yet we learn nothing about the prince's contacts with or financial support from his supporters in Britain. The role played in the expulsion by Madame de Pompadour, Louis XV's new mistress to whom Charles Edward had refused to pay court, and the subsequent damage to the Stuart cause from her lasting enmity are much underrated here.

EVELINE CRUICKSHANKS
University of London

ROBERT C. RITCHIE. *Captain Kidd and the War against the Pirates*. Cambridge: Harvard University Press. 1986. Pp. vii, 306. $20.00.

The present-day student of piracy has a difficult course to steer between the rocks of revisionism and the shoals of sensationalism. Robert C. Ritchie is a scholarly pilot, however, and although his vessel touches both these dangers—it summarizes some of the sillier recent essays and its narrative is occasionally overpainted—it gets hung up on neither. Ritchie overwhelms these dangers with data. Surely, nothing of consequence about Captain William Kidd remains to be discovered in Anglo-American archives in the wake of Ritchie's researches. He presents his findings with such clarity that this beautiful book—aptly illustrated, handsomely designed, and well produced—is the definitive account of a famous subject.

Why Kidd should be so famous and why he deserves such a scholarly study remain unclear. Kidd was a pirate, as Ritchie proves, but he was an unsuccessful and atypical sea robber, who operated under the weak shield of an unusual, antipiratical royal commission for a relatively brief period, at the end of piracy's most important period. Kidd sailed from New York on September 6, 1696, ostensibly to capture some of the Red Sea pirates who were outraging the politically influential East India Company of London. He returned to Boston on July 6, 1699, and was arrested by one of his Whig sponsors, the earl of Bellomont, governor-general of New York and Massachusetts.

In the interim Kidd had carefully confined his captures to "moorish" ships and to those carrying French passes, but he missed the pirates against whom he was commissioned. Fastidious, patriotic, and unsuccessful, Kidd was arrested, tried, convicted, and executed for piracy when far more offensive pirates were pardoned. His crime was political, not piratical. His Whig patrons feared that he would involve them in disreputable dealings with pirates. The Whigs' Tory enemies resented Kidd's failure to do so. Thus, Kidd's fame and Ritchie's book are more reflective of politics than of piracy. Kidd was trapped either by a conscience rare in this age of outrageous informers or by a fatal slowness to recognize the political price of pardon or by both. "The fellow is a fool as well as a rogue," said one of Kidd's would-be manipulators. Kidd was therefore found guilty of an irrelevant murder and an incidental piracy and hung in chains because neither political party would intercede with the king on his behalf.

The old sea rover had become a political liability because the ever more censorious English legislators had at last become aware of the incompatibility of traditional English piracy with modern international commerce. A Whig spokesman explained that "parliaments are grown into the habit of finding fault, and some Jonah or other must be thrown overboard, if the storm cannot otherwise be laid. Little

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men are certainly the properest for these purposes." Ritchie concludes that "Kidd was a very little man," sacrificed to the parliamentary politics of commerce and to the determination of the English executive to erase all challengers to its monopoly of violence (p. 192).

STEPHEN SAUNDERS WEBB
Syracuse University

CHARLES B. ARTHUR. *The Remaking of the English Navy by Admiral St. Vincent—Key to the Victory over Napoleon: The Great Unclaimed Naval Revolution, 1795–1805*. Lanham, Md.: University Press of America. 1986. Pp. xi, 265. $13.75.

Charles B. Arthur's thesis is that Britain's naval supremacy in the nineteenth century was a direct consequence of the reforms instituted by John Jervis, earl St. Vincent, in the English fleets of the late 1790s. The vigorous leadership of St. Vincent was initially directed at the reform of naval discipline and the revitalization of the Mediterranean fleet, including the elimination of patronage and jobbing in its supply. Together, these changes contributed to the great victories of St. Vincent and his captains, most notably that of Horatio Nelson on the Nile in 1798. St. Vincent's early successes enabled him to assume the primary role in Admiralty affairs, first as commander of the Channel fleet and subsequently as first lord, in the period leading up to the Peace of Amiens. In fact, Arthur argues that the adoption of St. Vincent's plan for the close and continuous blockade of Brest forced Napoleon to sue for peace in 1802.

These events and organizational reforms are well documented in this account, which brings them into clear focus and traces their background in the context of French naval activities of the period. One of the strengths of the book is that the author has consulted the French sources and has woven the French tale fully into the account of British activities.

One wonders, however, whether these developments, important as they were for both tactical as well as strategic reasons, constituted a naval revolution. That the supremacy of the British fleet was established in part because of these activities cannot be denied. Yet the ineptitude of the French naval command and its allies played a greater role than is acknowledged here. Furthermore, Arthur's case for the centrality of St. Vincent's role, though interesting, is not sustained by systematic and careful analysis of the internal documents of the British government. These documents were frequently consulted with great success on particular events. But they do not consistently inform his line of reasoning. The author has overstated his case and at times approaches polemic in criticizing other historians for not drawing the same conclusions.

This volume, in summary, significantly contributes to our understanding of the technical changes made in fleet management for which St. Vincent is justly acclaimed. It raises more questions than it resolves, however, about the development of grand strategy and the enhanced role of the navy in determining Britain's global supremacy of the nineteenth century. To sustain the central thesis, more attention would have to be paid to the political milieu in which the navy operated than was possible in this focused study.

CHARLES R. MIDDLETON
University of Colorado

MARY B. ROSE. *The Gregs of Quarry Bank Mill: The Rise and Decline of a Family Firm, 1750–1914*. New York: Cambridge University Press. 1986. Pp. xv, 169. $34.50.

This is an engaging and carefully researched study of five generations of a family firm of cotton manufacturers—the firm founded by Samuel Greg, near Manchester, in 1784. Mary B. Rose traces the firm's history from its early expansionist decades. She analyzes its stagnation during the middle years of the nineteenth century and its decline, which began in the last years of the nineteenth century. As an illuminating case study of a single enterprise, the book is a success. Rose not only provides insights into the nature and changing fortunes of this textile firm but offers a good analysis of the character of the Gregs themselves, beginning with the resourceful founder, Samuel Greg, who established a mill each for four of his sons. The succeeding generations of Gregs appear in Rose's story, along with the changing fortunes of their enterprise, and their story vividly illustrates the rise, domination, and decline of British cotton textile manufacturing over more than two hundred years. The story ends with a brief description of the textile museum that Quarry Bank Mill has recently become.

Rose demonstrates that the history of this firm both supports and challenges several common assumptions about the history of British industrial development. For example, Rose uses her case study to analyze the nature of entrepreneurship in the founding and succeeding generations and concludes that, in the case of the Gregs, easy generalizations about the comparative weakness of the skills of second-generation entrepreneurs in family firms are of little value.

Although Rose's book is very good, an extended analysis would have enriched the story in several places. For example, the book provides tantalizing glimpses of information that might illuminate ques-

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tions of interest to historians of the family. Whereas Rose's main concern is the business enterprise, the family relationships—between parents and children, husbands and wives—clearly played a part in shaping the business, and the business, in turn, clearly influenced the personal relationships. One would have liked to learn more, for example, about Samuel's "liberal and humanitarian" wife Hannah, who influenced the establishment of such amenities at Quarry Bank as a school and a "Female Society."

In conclusion, then, this study makes a valuable contribution to business history and to the social history of British enterprise, but the extensive research on which the book is based could probably sustain a deeper exploration of aspects of social history that are merely touched on in the present account.

DEBORAH GORHAM
Carleton University

ANNE DIGBY. *Madness, Morality, and Medicine: A Study of the York Retreat, 1796–1914.* (Cambridge History of Medicine.) New York: Cambridge University Press. 1985. Pp. xvi, 323. $39.50.

The York Retreat has long occupied an honored place in accounts of the institutional treatment of madness. Why a small, private, and at first religiously exclusive asylum should have gained such fame throughout the English-speaking world is itself a question worth addressing. But Anne Digby offers considerably more. Digby's quantitative analysis of the 2,011 patients admitted to the retreat between 1796 and 1910 combined with a judicious mining of case histories, annual reports, and contemporary medical literature allows us to penetrate the daily lives of both the keepers and the kept.

When Quaker philanthropists opened a retreat for mentally disturbed Friends at the end of the eighteenth century, they began with compassion for their troubled coreligionists but no practical experience in treating the insane. The therapeutic techniques favored at the start—warm baths for melancholics, leather-padded bolts on bedroom doors to reduce frightening noise, restricted use of the strait jacket—were deployed in the expectation that kindness rather than coercion would best heal sick spirits. This Quaker institution was not unique in deemphasizing mechanical restraint. But, thanks to the publication of Samuel Tuke's *A Description of the Retreat* in 1813, a broad audience was introduced to the experiment at York. Several new asylums in Britain and America subsequently tried to reproduce the experiment, but, in the long term, Digby argues, the retreat's contribution was not so much to act as a model as to "serve as a symbol of a new orthodoxy in the care of the insane" (p. 258).

By accepting Tuke's *Description* as representative of the retreat through time, however, latter-day accounts of its work have distorted historical reality. As Digby is at pains to show, by the early twentieth century the retreat, though still small (one hundred ninety patients in 1914), catered to a clientele that was neither poor nor predominantly Quaker. Moreover, lay therapy at York gradually gave way to a medical regime. The process by which Victorian doctors "captured" the treatment of mental disorder will be familiar to those who know the scholarship of Roger Cooter and Andrew Scull. To their insights Digby's case study adds ample proof that the triumph of the mad-doctors did not necessarily spell doom for the eclectic approach to treatment. Indeed, a willingness to improvise remained vital precisely because the retreat's medical superintendents failed to achieve the high recovery rates at which they aimed.

And what of the patients themselves? Digby gives short shrift to the whiggish view of the retreat as a paradise for the insane. But she also rejects Michel Foucault's equally sweeping stereotype of this asylum as a repressive institution hiding behind a mask of humanity. The private papers of William Waller, an attendant in the retreat from 1844 to 1856, suggest that at least some of those who dealt most intimately with the mad felt a deep sympathy for them. Implicit in Digby's analysis is the notion that some patients may have been inspired to recover mental health by their keepers' faith in the "divine spark" animating all humans (p. 28). Her failure to pursue such intriguing speculation is a disappointment, one of the few to be encountered in this fine book.

GEORGE K. BEHLMER
University of Washington

MICHAEL E. ROSE, editor. *The Poor and the City: The English Poor Law in Its Urban Context, 1834–1914.* (Themes in Urban History.) New York: St. Martin's. 1985. Pp. xi, 175. $29.95.

This collection of essays presents the nineteenth-century poor law system from the middle-class perspective of guardians, local officials, ratepayers, and politicians. Michael E. Rose, in an effective introductory essay, defends the concentration on the city as a way of restoring balance to a historiography still dominated by studies of the rural poor law. Moreover, these essays offer much of value to those with a more general interest in the history of the city.

Peter Wood's essay on the financial aspects of poor relief is based on a close study of the Sunderland Union between 1836 and 1914. He offers an admirably clear and concise explanation of the complexities of the rating system and the various

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attempts to reform it. Major iniquities regarding poor rates bedeviled the system, not only between town and country unions but also between central and outlying townships in the same urban union. Wood's essay makes clear that harsh relief policies did not necessarily result from vindictiveness or cruelty; they were often the consequence of harried boards of guardians trying to make ends meet on financial resources that were easily overwhelmed during economic crises. Similar insights are gained from David Ashforth's essay on settlement and removal in Bradford. The uncertainties and vexations of the law of settlement, which Adam Smith complained of, were by no means eliminated in 1834. Settlement litigation continued, but far more common were agreements by parochial authorities to provide relief to their settled poor resident elsewhere rather than bear the expense of removal. Ashforth leaves us with a picture of boards of guardians or individual parishes warily eyeing other boards and parishes, threatening and sometimes using their ultimate weapon of removal—from which, as always, the poor were the greatest sufferers.

Keith Gregson examines the relationship between the poor law and organized charity in northeast England, an area virtually ignored in earlier studies. Sections of the Charity Organization Society (COS) were active in the region, and many of them labored to achieve the efficient division of labor with poor law authorities outlined in the Goschen Minute of 1869. The results fell far short of expectations, however, and Gregson reveals that the problems of "exceptional distress" tended to overwhelm the tidy division of labor envisioned by COS activists as well as by some guardians. In the final essay Pat Ryan shifts our attention from the north to the capital and describes politics and poor relief in East London around the turn of the century. It is a useful companion piece to Gregson's essay, for Ryan examines COS control of the boards of guardians in Whitechapel, Stepney, and St. George's in the East, (which involved the abolition of outdoor relief in those unions), comparing it to the radically controlled Poplar board, where "lax" relief policies prevailed from the 1890s. His careful study raises as many questions as it answers, and one is made aware of the pitfalls of assuming that progressives or conservatives offered clear alternatives on relief policies.

ANTHONY BRUNDAGE
California State Polytechnic University

PHILLIP THURMOND SMITH. *Policing Victorian London: Political Policing, Public Order, and the London Metropolitan Police.* (Contributions in Criminology and Penology, number 7.) Westport, Conn.: Greenwood. 1985. Pp. x, 229. $35.00.

The origins and early history of the British police system, both in London and elsewhere, have received much attention in recent years from scholars on both sides of the Atlantic. Phillip Thurmond Smith's study narrowly focuses on the Metropolitan Police during the eighteen years Richard Mayne was virtually sole commissioner, 1850–68, and even more specifically on two police roles Smith terms "political": surveillance of suspect aliens and crowd control.

Under the first heading Smith elucidates for the first time the police role in watching individuals politically rather than criminally suspect, most of whom were Continental political refugees. The usual activity here involved housewatching, and, when opportunity availed, impersonation as a means of eavesdropping. Smith provides some new information on spies, detectives, and informers. The evidence suggests little sophistication, and, consequently, alien watching proved disappointingly unproductive and unsensational. Here, Smith's research brings to life one of the department's most effective but little-known detectives, John Sanders, whose extended observations led to a more relaxed governmental view of the danger posed by these people. More sensational is the revelation of a secret governmental program of using Foreign Office Secret Service funds to subsidize voluntary emigration of radical aliens to New York. The funds mostly were offered to French refugees from the Second Empire. The new evidence suggests a less-than-perfect adherence to the much heralded British doctrine of political asylum.

The section on crowd control centers on police response to four incidents: the Sunday Trading Disturbances of 1855, the Garibaldi Riots of 1862, the Reform Demonstrations of 1866–67, and the Fenian scare of 1867–68. In each case a demagogic press pandered to popular condemnation of the police as either too brutal or too indulgent, but Smith's research corroborates the prevailing view that the home secretary on a policy level and the commissioner on an operational level did their best to maintain that undefinable balance between the demands of public order and the preservation of popular rights of public assembly and free speech. Although not averse to taking a few leaves from historians of the crowd such as George Rudé, Smith has written an institutional history, not a social history, and he focuses throughout on police perspective rather than faces in the crowd. But each of these incidents is well known and, although Smith describes events in graphic hour-by-hour fashion and with the skill of a consummate story teller, little

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here cannot be found in the works of Charles Reith, Jenifer Hart, F. C. Mather, and other scholars.

Copious citations to Home Office files, Metropolitan Police files at the Public Record Office, newspapers, and personal correspondence attest the most industrious research. Inevitably, occasional lapses occur, as on page 192 where a passage from a letter from Lord Derby to Benjamin Disraeli is cited to a secondary source, Leon O'Broin, instead of the Hughendon Papers (B/XX/5/457), where the original letter may be found. The Hughendon Archives are also curiously absent from the otherwise exhaustive bibliography.

In spite of these purely academic reservations, the general reader interested in either mid-Victorian London or police history will find here an extremely good read.

DONALD RICHTER
Ohio University

ALON KADISH. *Apostle Arnold: The Life and Death of Arnold Toynbee, 1852–1883*. Durham, N.C.: Duke University Press. 1986. Pp. xiv, 285. $42.50.

Alon Kadish's biography of the man who gave his name to Toynbee Hall is a lucid and gracefully written study of Arnold Toynbee's life and death. In many ways it was the shortness of Toynbee's life—he died when he was thirty—that secured his position in English history and that made his myth as compelling as his contributions to economic history. Although Kadish signals this irony in his preface and especially in his very interesting final chapter, entitled "Death and After," he does not venture into as profound an analysis of it as the subject seems to invite. The biography reads, at times, like a nineteenth-century life of a great man: it is respectful and restrained and approaches the personal realm with more tact than a late-twentieth-century reader finds necessary. The book also stops short of fully explaining the author's crucial claim that Toynbee is "representative of a deeply troubled generation of Victorian middle-class, intellectual liberals" (p. ix), for Toynbee's life merits attention as an expression of the crisis and confusion of nineteenth-century liberalism.

Toynbee's father, who named his son after Thomas Arnold of Rugby (other sons were named for Wordsworth and Coleridge), was a successful aural surgeon, an associate of the sanitary reformer Edwin Chadwick, and a friend of Mazzini. Toynbee's mother was an apparently cold woman whose emotional remoteness from her son was reproduced later in the "mutual detachment" of Arnold's marriage to Charlotte Atwood. After the sudden and premature death of his father Toynbee experienced a period of depression and aimlessness before landing at Oxford, briefly at Pembroke College and then, momentously, at Balliol.

Kadish's chapter on Toynbee as "a Balliol man" makes clear that the story of late-nineteenth-century liberalism is, in many ways, also the story of university culture: the University Settlement House movement, to which Toynbee left his name if not his real intellectual legacy, is testimony to that fact. At Oxford Toynbee came under the influence of Benjamin Jowett, John Ruskin, and, most importantly, T. H. Green, and he imbibed the ethos of community service that Ruskin's undergraduate road beautification scheme in North Hinksey exemplified. This particular ethos, a combination of political, economic, and religious reformism, is perhaps best articulated in Toynbee's words on approaching graduation: he wrote to a friend that as soon as he got his degree his "life as a political economist and social agitator and philanthropist begins . . . I do really hope then to be a good soldier of Christ" (p. 52).

Between graduation from Oxford and his early death Toynbee worked at the lectures that make up his *Industrial Revolution* and at those on Henry George's *Progress and Poverty* that directly preceded—and some say precipitated—his death. In the former he established the significance of the term "Industrial Revolution" and helped create an explicitly historical economic discourse. He also put forward his philosophy of "radical liberalism," in which he tried to reconcile the values of self-help and free competition with the need for state intervention in the social realm. His ambivalence about collectivism emerges clearly in his two lectures on George, the first of which condoned the end of an "era of free trade and free contract" (p. 205) and the second of which enraged George's followers by roundly condemning the nationalization of land.

It was Toynbee's unresolved ambivalence about the optimal degree of state intervention and his confusion about the place of individualism in a society determined to alleviate social misery that made him a quintessential late-century liberal. Kadish is very good on Toynbee's virtual canonization by friends and protégés after his death. He might have done more, however, to place Toynbee in a wider political and philosophical context and to pursue his own suggestion that Toynbee's myth derives from his typicality rather than from the specific contributions of his work.

DEBORAH EPSTEIN NORD
Harvard University

TREVOR LUMMIS. *Occupation and Society: The East Anglian Fishermen, 1880–1914*. New York: Cambridge University Press. 1985. Pp. xii, 210. $39.50.

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This study of a single occupation in one region of England is both narrower and wider than its title suggests. Its geographical focus is generally limited to two English ports (Yarmouth and Lowestoft), which are less than ten miles apart. Yet it uses some interesting methodologies that enable it to address historiographical issues of great significance.

A protégé of Paul Thompson, Trevor Lummis has become a skilled practitioner of the latest oral history techniques. Many of the book's arguments are, in fact, derived from interviews with sixty individuals who were involved in the East Anglian fishing industry from the 1880s to the 1910s. While carefully preserving the informants' anonymity, the author provides a great deal of statistical information about them. He also includes many well-chosen quotations that reveal their perceptions of their lives and labors long ago. Many of these findings are then confirmed or qualified with evidence from parliamentary papers, contemporary newspapers, and trade journals.

Lummis contributes to debates over "working-class" formation by pointing to the importance of internal divisions among fishermen. The deep-sea driftermen, the trawlermen, and the inshore fishermen constituted three distinct occupational categories whose members rarely interacted with one another. In a fascinating chapter entitled "Images of Social Structure" (chap. 7), Lummis demonstrates that an individual's conception of society was heavily influenced by the particular type of fishing in which he was employed. Moreover, fishermen appear to have had little consciousness of being part of a larger "working class." Weak trade unions and the concomitant paucity of labor disputes among the East Anglian fishermen provide part of the explanation for this phenomenon. So, too, does the weakness of the early Labour party in the region. By contrast, Conservatives and Liberals appear to have attracted equally high levels of support from the fishermen until 1914.

The author deals with a number of additional topics, only a few of which can be mentioned here. For instance, he portrays family life among the fishermen as consisting of relatively placid partnerships in which husband, wife, and children "formed an effective economic and emotional group" (p. 132). As elsewhere in Britain, women might work outside the home, rent rooms to lodgers, take in laundry, or otherwise supplement what was regarded as the "family income." Heavy drinking was rare among the fishermen, whereas various social activities with their families were quite common. In religious matters, however, family cohesion was weaker. Although children and adolescents attended Sunday schools and religious services in moderately high numbers, their parents' attendance at services was relatively infrequent. Superstitious beliefs and practices flourished in this milieu, which consequently resembled the parts of Lincolnshire and Yorkshire recently studied by James Obelkevich and Deborah Valenze.

Lummis's book is a microstudy, to be sure, but many historians of late Victorian and Edwardian England will find it useful because of the ingenious techniques he uses to explicate a wide variety of important social and economic topics.

ROBERT GLEN
University of New Haven

DAVID RUBINSTEIN. *Before the Suffragettes: Women's Emancipation in the 1890s*. New York: St. Martin's. 1986. Pp. xvi, 272. $32.50.

Historians have written exhaustively about the question of women's suffrage in Great Britain in the first two decades of the twentieth century. David Rubinstein is probably the first to devote a book to the question of women's emancipation in the 1890s. He argues that the suffragette decade, which came immediately before World War I, should not be regarded as a sharp break with the past, unrelated to preceding movements, arguments, and opinions. Social historians are likely to agree with this point of view but may well ask why Rubinstein has concentrated only on the 1890s. He states "certain subjects lend themselves more readily to intensive examination than others. The history of English women in the 1890s is one such subject" (p. x). But one could equally well argue that he has chosen an artificial period; for example, in his chapter on women's legal image he concentrates on the Jackson case of 1891 in which the Court of Appeal established that a husband could not forcibly imprison his wife. To take a longer period and include in this chapter other important legal reforms that improved women's rights, for example the earlier child custody and married women's property acts, might have been more sensible. Similarly, the struggle to improve women's working conditions and to extend their job and career opportunities predated the 1890s. Above all discussion of women's suffrage began long before 1890. John Stuart Mill's speech to the House of Commons in 1867 brought the question close to the center of the political arena. Annual bills for women's suffrage were introduced from 1871 to 1883, but all were lost. Outside Parliament women were active; in 1867 three local societies joined together to form the National Society for Women's Suffrage. The society grew and the debates continued through the 1870s and 1880s. What then was particular to the 1890s? At the national level women made little progress politically but, after legislation in 1894 reduced the property qualification, the number of female Poor Law Guardians

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and urban and parish councilors greatly increased. Women also served in increasing numbers on borough education committees. Rubenstein acknowledges that actual gains were not very significant but argues, convincingly, that the rights of women were debated as never before. Their rights in the spheres of employment, politics, law, education, marriage, the family, and society as a whole were widely and avidly discussed. A new spirit was born, which provided fertile ground for the later growth of the militant suffrage movement.

Whatever one's reservations about Rubinstein's choice of period, there is much to delight in this book. It is a rich and fascinating source of information, culled from a wide variety of sources, about the often lively and always interesting women who led the struggle for emancipation—academics, professional women, writers, factory inspectors, and so many more. This book is both very readable and very enjoyable.

ELIZABETH A. M. ROBERTS
University of Lancaster

MICHAEL ROSENTHAL. *The Character Factory: Baden-Powell and the Origins of the Boy Scout Movement.* New York: Pantheon. 1986. Pp. x, 335. $22.95.

Michael Rosenthal's study of the Boy Scouts is "an attempt to 'read' the culture of Edwardian England through one of its representative institutions and the man who started it" (p. 1). Scouting, Rosenthal asserts, was initially successful largely because it addressed the concerns that most deeply worried the Edwardian governing class: "national efficiency," juvenile delinquency, labor unrest, the stunted physical condition of the urban underclass, and the anticipation of war with Germany. Robert Baden-Powell offered discipline, character building, and fresh-air recreations for children of all classes. He tried to impress public-school values on the working-class boy, teaching him to "play the game," to be "loyal to the King, and to his officers, and to his country, and [significantly] to his employers" (p. 109). Scouts were not required to drill, because Baden-Powell realized that many parents would be alienated by any hint of militarism, but his clear objective was to prepare British youth to fight the next war.

Rosenthal's premise—that the Scouts were "representative" of Edwardian society as a whole—is a somewhat reckless generalization. To say that Scouting reflected the values of conservatives, as well as some liberals and socialists, would be more correct. Rosenthal also tends to imply that the views of the movement and its founder were identical, which was not always the case. The Scouting organization, for instance, did not share Baden-Powell's enthusiasm for Mussolini's Balilla and the Hitler Jugend. This book, in fact, tells us little about the attitudes of other Scouting officials or of the Scouts themselves. Statistics that strikingly illustrate the popularity of Scouting are readily available, but they are not reproduced here.

A more serious objection is that most of Rosenthal's discussion is already familiar to scholars. Much the same ground has been covered by John Springhall in *Youth, Empire, and Society: British Youth Movements, 1883–1940* (1977), Samuel Hynes in *The Edwardian Turn of Mind* (1968), W. S. Adams in *Edwardian Portraits* (1957), and by Paul Wilkinson and Michael Blanch in articles. All too often, the same data, anecdotes, quotations, and examples cited by these authors are reused in the present book. Rosenthal's examination of pre-1914 invasion stories closely follows Hynes, and his debunking of Baden-Powell's record in the Boer War is derived largely from Brian Gardner's *Mafeking: A Victorian Legend* (1966).

Rosenthal has seriously scrutinized the Chief Scout's papers. The book is best when it shows how Baden-Powell invented the Boy Scout with an eye toward attracting the largest possible following. The Scout was a clever compromise, falling somewhere between a forester and a cadet, who at once upheld the values of empire and of international brotherhood. That mix helps explain why the Boy Scouts were more popular than paramilitary corps such as the Boys' Brigade and vastly more popular than antimilitarist youth groups such as the Woodcraft Folk. As a colorful distillation of more scholarly works, this book can certainly be recommended to laypeople and undergraduates. But it should not be assigned to graduate students, except as an example of what does not constitute original research.

JONATHAN ROSE
Drew University

J. M. WINTER. *The Great War and the British People.* Cambridge: Harvard University Press. 1986. Pp. xiv, 360. $25.00.

A more accurate, if wordy, title for this impressive book might be "The Great War and the Health and Living Standards of the British Working Class." J. M. Winter's contention, developed previously in a number of articles that reappear as chapters in this work, is that the war years marked a period of substantial improvement in the physical well-being, longevity, and general quality of life for the civilian, laboring population. He has skillfully and imaginatively analyzed demographic data extracted from parliamentary papers, vital statistics, health and census reports, and the life tables of the Prudential Assurance Company. Considering them together

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with a wide range of unpublished manuscript sources and private papers, Winter concludes that, however dreadful the military losses suffered by the country, they were more than offset by the economic, physiological, and, ultimately, demographic advances enjoyed by those who remained at home in the work force. In Winter's opinion World War I was won not at the expense of the health of the civilian population but because of it.

Winter carefully bases his argument on a statistical and social analysis of the civilian and military population and the declining mortality of men, women, and, most important, children. He explores such subjects as the availability of medical care, trends in wages and family income, food prices, housing and working conditions, and the effects of social welfare and health administration on the civilian populace. In sustaining his claim that the demographic consequences of the war have too often been distorted by emotional recollection and collective memory, Winter also considers the patterns of expanding nuptiality and declining fertility that have characterized British society in the twentieth century. Many of his data are summarized in nearly seventy tables and graphs, which are, for the most part, clearly explained and whose inadequacies are candidly noted. They should not deter the statistical neophyte from this valuable and innovative social history.

In any event, the reader is unlikely to misunderstand Winter's central thesis, which is frequently repeated, or his explanation for the significant improvements he measures in the health and life span of the working classes and their offspring. By a process of elimination he concludes that they were caused by a critical rise in the standard of living resulting from full employment, rent control, and, most important, higher wages and earnings that eclipsed inflationary food costs and contributed to a marked improvement in nutrition. Despite shortages and rationing, he contends, the diet of the working class in general and, particularly, its poorest members strengthened their resistance to disease and substantially increased their health and longevity.

Although Winter acknowledges that perhaps some of the most important demographic issues are not quantitative but involve mentality, attitudes, and beliefs, he is least successful when he tries in a brief, closing chapter to bridge the gap between quantification and intellectual and cultural history. The strength of this thoughtful, highly original book is in its statistical accounting of the demographic impact of World War I, in both the staggering loss of lives and, paradoxically, the saving of a great many more.

RICHARD A. SOLOWAY
University of North Carolina

JERRY WHITE. *The Worst Street in North London: Campbell Bunk, Islington, between the Wars.* (History Workshop Series.) London: Routledge and Kegan Paul; Distributed by Methuen, New York. 1986. Pp. xii, 312. $19.95.

Jerry White declares at the outset of this volume that his object is not so much description or narrative as understanding. In this aim he remains true to one of the major purposes of the History Workshop Series and to the historical tradition of which it is a product: to write a history committed to the reconstruction of human lives and their coming together into class consciousness, "of sharing the same predicament, of being within a particular position within the social framework, and of being at odds with some other parts of society" (p. 28). Oral interviews are a major source of evidence, as they were in his earlier volume in the series, *Rothschild Buildings* (1980), accompanied in this instance by inquests and other court hearings, reports of the Metropolitan Borough and Ministry of Labour, and various other official and semi-official documents and records.

The result is a carefully objective yet frequently moving account of life in what was universally described as one of London's worst slums during the interwar period: Campbell Road or, as it was better known, Campbell Bunk. Those familiar with the anatomy of twentieth-century working-class neighborhoods such as Richard Hoggart's Hunslet and Michael Young's and Peter Willmott's Bethnal Green will immediately realize that White has brought them to a far different territory. The Bunk was home not to the working class but to what White identifies as lumpenproletariat: men and women consistently rejected by and content to reject the conventional labor market. Almost from its birth as an Islington suburb in the 1870s, the Bunk was part of "outcast London"; it remained one of the last outposts of what bureaucrats labeled "the unemployables" until World War II.

The result was a particular ideology, bred in "rough" lodging houses and furnished rooms and nurtured by equally "rough" and frequently illegal occupations. Compounded of egalitarianism, individualism, libertarianism, and chauvinism, this view of life, which White painstakingly and convincingly extracts from his evidence, sustained the inhabitants in the face of hostility from employers, civil servants, policemen, and the "respectable" working-class population, who perceived the Campbell Roaders as both a threat and a warning.

White has not attempted to generate nostalgia for the life lived in Campbell Bunk. Extreme poverty ate into the natural inclinations of its citizens to stand together against the outside world. They stole from each other while they stole from the precincts beyond their own road. Men deprived of a chance to

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assert themselves as traditional breadwinners did so instead by beating each other and their wives and children. "I don't want to think of it, it's terrible when I think of it" says one resident of the brutal fights between her parents (p. 139). Yet, despite the consistently grim details, one cannot help but agree with those who remember it that the Bunk possessed a fascination—"the Hyde lurking in every respectable street of Jekylls," White calls it (p. 237). The reader gains the sense that these were tough people, tough on themselves and on the society for which they had little use; they were tough as well, however, as survivors in prisons and on desert islands are tough—they created their own society, with its own ethic and humor, as the only way to exist in a world over which they had little power.

STANDISH MEACHAM
University of Texas

DENIS SMYTH. *Diplomacy and Strategy of Survival: British Policy and Franco's Spain, 1940–1941*. New York: Cambridge University Press. 1986. Pp. xi, 335. $49.50.

Denis Smyth's commendably comprehensive study of British diplomacy toward Spain in the crucial years 1940–41 examines one corner of that black period to draw the clearest picture possible of the unpalatable choices that faced the beleaguered British government. Winston Churchill, of course, had always nurtured a degree of sympathy for the ideological aims of the Spanish Nationalists and was especially conscious of the strategic importance of Spain to the British and later the Allied cause. But some members of the wartime coalition faced decisions demanding a pragmatism only the extremity of the situation made tolerable. Anthony Eden, foreign secretary from December 1940, Labour leader Clement Attlee, and Labour Minister of Economic Warfare Hugh Dalton (who considered British policy toward Spain to be one of "sheer abasement" [p. 60]) balked at the accommodation afforded to Francisco Franco.

Many crises had to be overcome in Anglo-Spanish diplomacy. During the summer of 1940 when Britain's survival was in doubt and the chance of keeping German troops out of Spain seemed slight, Samuel Hoare proposed, Halifax seconded, and Churchill reluctantly agreed that an offer be made to discuss the future of Gibraltar with the Spaniards at the close of hostilities. Throughout the war the British employed the threat of embargo and promise of aid to keep Franco at least nominally neutral, and they were not, as Smyth tantalizingly indicates, above bribing a group of Franco's disaffected generals into opposing the more extreme elements. But equally important seem to have been the limitations on Hitler's choices, which by the spring of 1941, lay, it is suggested, between Spain as a *point d'appui* for the western Mediterranean and the pressing need to control the Romanian oil fields. The token forces of the Blue Division that Franco dispatched to the Russian front were, paradoxically, useful to the British in that they allayed Hitler's doubts as to the sincerity of Franco's allegiance and thus rendered a German attack on Spain less likely.

In an area in which historians have hitherto had to rely on official histories, or memoirs, this study based on previously unpublished sources offers a detailed and scholarly account of the problems that Spain presented for the British at a time of crisis. Smyth raises issues that remain alive in the postwar world, and his book will therefore have much to offer any reader with an interest in the complexities and dilemmas of decision making under the most extreme conditions ever to have faced a British government.

JILL EDWARDS
Queen Mary College
University of London

RONALD M. SUNTER. *Patronage and Politics in Scotland, 1707–1832*. Edinburgh: John Donald; distributed by Humanities, Atlantic Highlands, N.J. 1986. Pp. vii, 254. $39.95.

Ronald M. Sunter's book begins with five chapters on the importance and availability of various types of patronage in Scottish electoral politics. He then presents a series of case studies of the central role of patronage, and sometimes outright bribery (especially in burghs), in four county and three burgh district contests. His thesis, which is shifting and elusive, is that patronage was an essential and omnipresent tool of electoral management but that, especially in the counties, such management was not necessarily successful unless the patron had personally cultivated his electoral interest and established a sense of "friendship."

This is hardly a revelation. Despite differences of degree, the same things might be said of English electoral politics, and everyone has always known this to be true. Sunter begins by erecting a straw man. He has the mistaken impression that historians of Scottish electoral politics have believed, or have given the impression, that patronage and bribery were the "all in all" in the counties and burghs and that a sufficient number of electors were "in the pocket of a political manager" to carry most elections (p. 2). This is a grave distortion of current views. It is perhaps significant that Sunter attributes them to no one. Indeed, his book does not include any historiographical discussion whatsoever. His footnotes are restricted to primary sources. If he

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wished to redress an imbalance in accepted interpretations, as he says, then he has failed to do so by default. He largely ignores the great managers, whom he rightly notes have received the focus of historical attention. One comes away with the impression that he does not really understand the role of the managers. Certainly, he fails to recognize that of Sir Thomas Dundas, despite his close attention to the Dundas papers.

This could have been an important book. Patronage, which has long been a seriously neglected subject, has only recently begun to receive suitable attention. Sunter provides interesting case studies and miscellaneous information. Historians will mine it and cite it for these qualities. But for interpretation we must look elsewhere.

DONALD E. GINTER
Concordia University

SHARON KETTERING. *Patrons, Brokers, and Clients in Seventeenth-Century France*. New York: Oxford University Press. 1986. Pp. x, 322. $36.00.

Sharon Kettering, already known as an authority on French royal administration and politics during the first hundred years of the Bourbon monarchy, in this volume makes a signal contribution to our understanding of that important period.

Continuing to make extensive use of the Provençal archives she has mined effectively in the past, Kettering here draws as well on Parisian collections and on documentation found not only in England but also in Monaco and even the United States (specifically, at Chicago's Newberry Library). In addition, a careful survey of published sources and secondary literature has enabled her to introduce supporting evidence from five other provinces in southern and eastern France: Guyenne, Languedoc, Burgundy, Franche-Comté, and Alsace.

The book's purpose is set forth with admirable directness at the outset. It is to investigate a relationship "in which patrons provide material benefits and protection, and clients in return provide loyalty and service" (p. 13). Needless to say, this exchange played a vital role at the center, the royal court itself, but it was if anything even more significant for the outlying provinces, where an individual's advancement often depended on the advocacy only a well-placed patron could supply. The transaction usually required in addition the help of a third party, the broker who put client and patron in touch with one another. And so the triangular bond was formed.

In a series of solidly documented chapters, Kettering explores the system as it evolved from its sixteenth-century form, through Richelieu's and Mazarin's systematic use of their *créatures* (and of the latters' clients in turn), to the emergence of recognizably bureaucratic administration under Le Tellier, Colbert, Le Tellier's son Louvois, and other major ministers of Louis XIV. By the time the study comes to an end, its author has examined not only the shape and influence of various types of provincial clienteles but also the effects of brokerage on intendants, parlements, nobles in general, and, of course, the crown itself. The result is a volume sharp in its focus but by no means narrow in its implications.

Over the course of the past forty or fifty years, works such as Roland Mousnier's on ownership of offices, Philippe Sagnac's on *la noblesse de fonctions gouvernementales*, and Orest Ranum's on Richelieu's methods of rule have progressively expanded our knowledge of how and where the king's writ ran, at some times better than at others, under the prerevolutionary French monarchy. Patronage was not a feature of the Old Regime unknown to modern scholarship, but Kettering has now provided a careful analysis of the practice, which served as at once a glue and a lubricant for virtually all parts of Bourbon government in the *grand siècle*.

Especially impressive is her success in portraying both useful and corrupting aspects of the patron-broker-client relationship. On the one hand, she convincingly argues that, without patronage, what amounted to a Renaissance court bequeathed by Henry of Navarre could not have kept pace with an expanding and increasingly complex nation. On the other hand, as the seventeenth century progressed, dealers in influence posed serious difficulties for exponents of rational public administration—as lobbyists still do today. One of Kettering's conclusions, namely, that "clientelism did not disappear as the . . . royal bureaucracy developed but adapted to it, coexisting comfortably inside and outside the new political system," Colbert himself could scarcely have denied (p. 9). After all, he had never failed to take advantage of it when opportunity arose.

FRANKLIN L. FORD
Harvard University

MICHÈLE ROOT-BERNSTEIN. *Boulevard Theater and Revolution in Eighteenth-Century Paris*. (Theater and Dramatic Studies, number 22.) Ann Arbor: UMI Research. 1984. Pp. ix, 324.

The history of the Parisian theater just before and during the French revolution has been the subject of numerous studies. As Michèle Root-Bernstein points out in her introduction, many of these studies, especially those about the "little" theaters on the boulevard, were done long ago. Marvin Carlson's more recent account, unfortunately the principal work in English, is largely based on these earlier works and repeats errors they contain. Also, histo-

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rians are now trained to look at the theater from an economic and social point of view and employ quantitative methods. For these reasons Root-Bernstein is justified in taking a new look at the boulevard theater at an important juncture of French history.

After an informative introduction Root-Bernstein treats the period from about 1760 to 1794 in four thematic sections, each preceded by a "prelude," a sort of impressionistic sketch of the aspect of theatrical life that she is about to probe. The first section deals with the official hierarchy in the theater established in the seventeenth century, the limitations this placed on the little theaters, and its modification through the ingenuity of the producers and the conflicting aims of different government agencies. She then turns to a fascinating analysis of the dramatic traditions of the boulevard, the love-intrigue, and the rise of the new moralistic drama. Her analysis of the relation of these to the hierarchical social order is especially insightful. The third section deals with the professional life of the actors and playwrights and is based on considerable new evidence. Since the authors of popular pieces dwelt in a world below that of the respected playwrights of the privileged theaters, she has the opportunity to see if it was a breeding ground of radicalism similar to that of the grubstreet writers described by Robert Darnton.

Many historians will await with expectancy the last fifty pages, which treat the response of popular playwrights, producers, and actors to the revolution. Here Root-Bernstein adduces new evidence about a subject treated many times before. She shows the playwrights of the little theaters did not lead the attack on the monopolies enjoyed by the royal theaters and the demand for authors' rights. Also, she finds that very few of the personnel of the world of boulevard spectacles became radicals; some supported the revolution, and most remained passive. Above all she demonstrates that only extreme pressure from the outside, especially the Commune of Paris, forced the little theaters to modify their traditional fare. In her treatment of the censorship of the theaters she corrects serious errors that previous authors have passed on, like the baton in a relay race, since Alexandre-François Vivien's study in the mid-nineteenth century. Probably in kindness to the memory of Beatrice Hyslop, she does not cite her attempt at a statistical study of the theater during the Terror.

Root-Bernstein quotes an observer who argued that even the physical layout of the traditional theater constituted an obstacle to its becoming a revolutionary medium. Recent research, of which Root-Bernstein is unaware, has shown that the revolutionary leaders were conscious of this. Not only did they order removal of the boxes in the Odeon to transform it into a Théatre de l'Egalité but in the great contest of Year II they called on architects to design theaters of a new kind. Unfortunately, Root-Bernstein does not give a full account of some of the revolutionary plays produced during the Terror, perhaps because she thinks that this has been done in earlier studies. Also, she terminates her account in 1794, denying us an account of the denouement after Thermidor. Nevertheless, her work provides many new insights into the political, social, and cultural life of the period, all presented with an artistic flair.

JAMES A. LEITH
Queen's University
Kingston, Ontario

MORRIS SLAVIN. *The Making of an Insurrection: Parisian Sections and the Gironde*. Cambridge: Harvard University Press. 1986. Pp. ix, 236. $27.50.

Morris Slavin's book on the Paris insurrection of May 31–June 2, 1793, represents an attempt to bring coherence to what he calls "this whole confused mess of a struggle" (p. 62). Georges Lefebvre lent it some distinction by calling it the best organized *journée* of the revolution. The reasons for the dissonance between the goals of its organizers and its immediate and long-term consequences, however, remain in dispute. Slavin borrows insights from some historians and rejects the explanations of others who have worked the field before him. His preferences are not in doubt. He deploys a methodology and a commitment strongly affected by the work of George Rudé and Albert Soboul. He adds to it his interest in the Enragés, the most miniscule and marginal of revolutionary groups, whose activities he raised from obscurity at about the same time as Walter Markov and R. B. Rose began to publish on them. The major modern historian whose knowledge of the revolutionary sublunaries and underworld is unequaled is not mentioned. Slavin plainly finds Richard Cobb's interpretation of them distasteful.

Slavin states that the battle between the Girondins and Montagnards in the Convention was an intraclass and not a class struggle on the grounds that no social class is homogeneous. Slavin deals only obliquely with what this might mean politically. Others might be content to speak of it as a struggle for power. But the real focus of his book is the mounting of the insurrection. Sectional politics and the politics of the Paris Commune have long rightly been held as holding the key to understanding its origins. Slavin is good but not decisive in charting the ups and downs of their agreements and divisions. A chapter on the membership of the key Comité central révolutionnaire, which wrested power from the Commune's Comité général

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révolutionnaire but did not completely displace it, furnishes interesting details about the men who were active in the various phases of the revolution but sheds little light on how they acted as organized political bodies. The prosopographical method might have been more productive. On the other hand, Slavin provides some incidental edification on the alarm with which both Girondins and Montagnards regarded the assault from below on the Convention. They saw it as a serious threat to the principle, not yet firmly in place, of national representation. The Convention's creation of the Commission des Douze to preserve its immunity proved ineffective against the *menu peuple* whose concerns and wishes, even if despised, were not taken lightly by most deputies, including Robespierre. His motives, however, were not always clear, probably not even to himself. The rhetoric of mutual contempt with which deputies and outsiders, including Jean-Paul Marat and Jacques-René Hébert, indulged their hatreds and expressed their political and social ideas abounded in references to masks and veils. The actors exhausted themselves in a competitive search for the "true" purpose of the revolution.

Slavin has conscientiously searched through the archives and demonstrated familiarity with the issues. The result is a thumping reaffirmation of the orthodox wisdom. The insurrection, we learn once again, saved the revolution; the beleaguered revolution was bourgeois; the time was not ripe for a social program elevating selflessness, ruthlessness toward profiteers, and egalitarian concern for the propertyless; and all this led to the end of the popular revolution, first in Thermidor, and then in Prairial. To complete the picture, Slavin indulges in some speculation. Although he admits (p. 143) the absence of documentary evidence, he accuses the Jacobins of using Claude-Emmanuel Dobsen and Louis-Pierre Dufournoy, key members of the Comité central révolutionnaire, to subvert its work. This cannot be entirely ruled out, but he is on safer ground in showing how the insurrectionary Comité des Neuf, enlarged to its membership of twenty-five, was the insurance the Jacobins took out to end a permanently volatile political atmosphere threatening the goals of the revolution as they perceived them.

HARVEY MITCHELL
University of British Columbia

CLAUDE PETITFRÈRE. *Les Bleus d'Anjou (1789–1792)*. (Commission d'histoire de la révolution française, Mémoires et documents, number 42.) Foreword by JACQUES GODECHOT. Paris: The Commission. 1985. Pp. 531. 290 fr.

This book is the second part of Claude Petitfrère's *thèse de doctorat*; the first part appeared in this same collection as *Les Vendéens d'Anjou* in 1981.

As the author points out, to devote a study to prerevolutionary patriots in a province that was a hotbed of counterrevolution may at first appear to be some kind of "joke" (pp. 35 and 471). Jacques Godechot, however, in his preface to this work notes that neither peasant insurrection nor its repression can be adequately comprehended without knowledge of this essential element in the situation. The major problem is to identify committed patriots, and the solution is the same as that used for the counterrevolutionaries: service in the armed forces of their respective causes. Petitfrère's study, therefore, is an exhaustive examination of the men who composed the National Guard in this region and, above all, of those who enlisted in the three battalions of national volunteers of the department of the Maine-et-Loire.

The book is a typical example of the research of French scholars on this period. It includes a comprehensive bibliography and a scrupulous evaluation of the primary sources and the methodology used. It provides painstaking—probably excessive—detail, including statistical tables, graphs, and charts, accompanied by hundreds of individual examples. Even the results of Petitfrère's research follow a not unusual pattern in that many of the conclusions are what we previously believed or expected them to be. For example, most of the national volunteers were urban in background, especially from the larger cities, Angers and Saumur; artisans and shopkeepers formed the backbone of these formations, although the upper and middle bourgeoisie provided a disproportionately large number of men and most of the leaders; and inhabitants of the countryside, particularly peasants, were much more readily found in the ranks of the "Whites" than among "Blues." The great difference is, of course, that we now have convincing evidence for our beliefs and expectations.

The author effectively integrates his findings about the national guardsmen and volunteers of the Maine-et-Loire into a broader context by comparing his findings with other local or, where possible, national studies and by contrasting this information with the data he has uncovered about the personnel in the counterrevolutionary forces of that department. As a result, this book provides a useful contribution to the literature about the National Guard and the national volunteers of 1791 and 1792; it takes its place among the major studies of the West, such as those of Paul Bois and Charles Tilly, which are informative not only about counterrevolution but also about the significance and complexities of the revolution itself, without which counterrevolution would not have existed.

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This is a book for specialists interested in the issues noted above, and even for them it includes extraneous materials, such as the data on the incidence of the first names of volunteers and the annoying presentation of percentages to the second decimal place (for example, 64.13 percent) when everyone, including Petitfrère, knows that the documents provide no such precision. Whatever the book's minor problems, no one claiming to be a specialist in the French revolution or in counterrevolution can ignore this meticulous study.

SAMUEL F. SCOTT
Wayne State University

CAROL BLUM. *Rousseau and the Republic of Virtue: The Language of Politics in the French Revolution.* Ithaca: Cornell University Press. 1986. Pp. 302. $24.95.

This is a fashionable book: title, thesis, and vocabulary proclaim it *à la mode*. The first half is given to an analysis of Rousseau, the second half to his ideas in the revolution. The book's strengths lie in the sometimes shrewd and subtle readings of Rousseau in psychosexual and cultural terms; its weaknesses begin with 1789.

Until recently literary scholars and critics have thought the writings of the revolutionary period unworthy or uninteresting. Carol Blum's declaration that she will treat the speeches, journalism, and correspondence of the revolution "as segments of discourse, on the same level of explicability as any other prose" is refreshing (p. 16). The utterances of the revolution are promoted to literature; the revolution itself is demoted. Blum cites virtually all the enormous recent scholarship on Rousseau. A recitation of some of the recent work on the revolution, Robespierre, Saint-Just, the Jacobins—all of which Blum treats at length—is impressive. She does not mention Richard Cobb, Michel Vovelle, Lynn Hunt, Jacques Ozouf, Patrice Higonnet, or Keith Baker. For the Jacobins she does not cite George Armstrong Kelly, Michael Kennedy, Claude Mazauric, nor even Crane Brinton or Gérard Walter. M. J. Sydenham on the Girondins is not mentioned, and the chapter on Louis XVI's trial does not mention Michael Walzer, Girault de Coursac, Alison Patrick, or even E. Seligman. The most recent work on Robespierre cited is Conor Cruise O'Brien's essay in *The New York Review of Books*; Norman Hampson, Marc Bouloiseau, and Max Gallo are not mentioned.

This apparent ignorance, indifference, or contempt leads to errors of detail and, more seriously, to the neglect of significant aspects of the revolution. Saint-Just was not executed on 9 Thermidor (p. 162), nor did the Committee of Public Safety fall on that day (p. 182). There was no Mountain in April 1792 (p. 169), and the Legislative Assembly—we are not told its numbers had dwindled drastically by August 10—did not try to incarcerate the king in the Temple (p. 170). Blum treats the Jacobins as indistinguishable from the Mountain and never mentions the two purges of the society. The struggle between Jacobins and Girondins in the king's trial is ignored, for this chapter is mostly devoted to an analysis of Saint-Just's two speeches: no mention is made of four separate votes. The institution of the Revolutionary Government and the Terror is not related to the Hébertist uprising, the foreign war, or the Vendéan uprising, nor is the Constitution of 1793 and the ascendancy of Robespierre and the Committee of Public Safety related to the purge of June 2.

For Blum all is text, not context. Her methodology excludes the need for historical precision and erudition. It also leads her to original conclusions. She cites, for example, George Rudé's analysis of Robespierre's behavior just before 9 Thermidor and says his view depends "on a Robespierre envisaged from the outside as seeking political power, a stable government, or the enactment of the republican constitution." In fact, she concludes, "as we have seen, Robespierre's own discourse explicitly repeats that his objective is *not* those ordinary aims but an entirely different goal, the achievement of a rapturous union in virtue with all of France" (p. 270).

Such a reading of the texts from the "inside" works with some success for Rousseau. His was an uneventful life passed in seclusion and self-absorption, and his autobiography, which he wrote over and over, fixed his thought in his life by establishing a dialectical relationship between acts and their depiction. But the lives of Saint-Just and Robespierre *are* the revolution. That stupendous event is not a static backdrop for the elaboration of ideas about virtue. And, oddly enough, Blum even ignores the potentially rich theme of psychosexual motives in Robespierre (and Saint-Just), although Gallo has written illuminatingly on the subject.

Mine is not a plea for the superiority of history over literary criticism. It is a plea for the integrity of a subject and the discipline of a craft. Whatever may be true for Rousseau, the texts of the French revolution do not breathe naturally when pulled from that turbulent sea. Revolutionary rhetoric, for all it shares with the rhetoric of other times, remains the rhetoric of the revolution.

DAVID P. JORDAN
University of Illinois, Chicago

ALAIN CORBIN. *The Foul and the Fragrant: Odor and the French Social Imagination.* Cambridge: Harvard University Press. 1986. Pp. 307. $25.00.

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In his controversial study of unbelief in the sixteenth century, Lucien Febvre (1942) suggested the need for a historical investigation of the five senses and their place in early modern mentalities. Accordingly, Alain Corbin has joined Robert Mandrou (1961) and Jean-Paul Aron (1967), among others, in writing an anthropology of past sensual perception. Specifically, the present book explores the historical implications of smell and "the modes of behavior that were caught up in the vague and contradictory theories formulated about olfaction" in the century before 1880 (p. 8). And it does so with an astonishing range of evidence and insight worthy of our attention and respect, however idiosyncratic or self-indulgent the topic may at first appear to the historical profession.

Corbin's essay examines a revolution in olfactory perception that contributed to modern sensibilities before the twentieth century. Scientists, officials, and notables of the Old Regime were only the first to develop a keen sensitivity, a special irritability rather, to strong odors. Influenced by new ideas about phlogistic air, urban planning, and social hierarchy, enlightened elites tolerated less and less the miasmic effluvia given off by all apparent putrefaction, the stenches of streets and courtyards in the early modern city, even the heavily scented perfumes made from musk, civet, and ambergris. The heightened perception of smell led to increasing attempts to deodorize air everywhere, especially in the space defined by the home and new institutions such as hospitals and prisons. In time the meaning of smell also changed. As men and women increasingly became narcissistic during the nineteenth century, the more subtle floral scents provided by the bourgeois enthusiasm for country gardens, greenhouses, and house plants developed new connotations of eroticism and social status. A middle-class culture based on this new sensibility became distinct from the world of stronger odors lingering among the peasantry, the working class, and the aristocracy. Corbin thus examines the role played by intensified olfactory sensation in three important fields of concern from the eighteenth to the nineteenth centuries—the physical sciences, urban reform, and cultural sensibilities—thereby broadening and deepening the significance of a historical development that would otherwise be overlooked.

Despite, or more accurately, because of its virtuosity of conception, research, and expression, Corbin's work is *haute vulgarisation* par excellence. The book dazzles in the most provocative Annales tradition. Consequently, the stimulating argument tends to hop unpredictably and contradictorily through the text. Corbin links modernization to both deodorization and individualism, even though individualism is also tied to a new range of delicate smells more expressive of the self (pp. 77–85). The book also uncritically accepts a process of cultural filtering from the top downward, notwithstanding the important influence of popular cultural traditions on romantic elites early in the nineteenth century (pp. 60, 199). Moreover, Corbin erratically chooses his evidence from widely dispersed periods and regions in France, but mostly from Paris, without comparative examination of similar developments earlier, later, and elsewhere in Europe. Occasional unfounded assertions appear in the text, such as the centrality of smell to modern sensibilities (p. 7) and the decisive role played by ventilation in the development of bourgeois narcissism (p. 95).

These criticisms aside, Corbin's book makes an intriguing contribution to French social and intellectual history. Like the most lasting achievement of Napoleon III, who, according to Edwin Chadwick, had "found Paris stinking and left it sweet," this essay on the changing perceptions and sensibilities of modern France deserves the attention of all scholars fascinated by intersections in the histories of science, cities, and culture.

JAMES SMITH ALLEN
Phillips University

JEAN-PIERRE GOUBERT. *La conquête de l'eau: L'avènement de la santé à l'âge industriel.* Foreword by EMMANUEL LE ROY LADURIE. ("Les Hommes et l'Histoire.") Paris: Robert Laffont. 1986. Pp. 302. 92 fr.

Did water conquer the industrial age? Or did the industrial age conquer water? Perhaps both, according to Jean-Pierre Goubert's study of the water supply and the changing demand for water in France during the nineteenth and twentieth centuries. Between 1780 and 1890 scientific discoveries that linked contaminated water to disease, particularly the discoveries of the 1880s that tied it to cholera and typhoid, intensified authorities' concerns about how best to provide pure water and increased public awareness of water's importance. In public sanitation, after many detours, the principles of building water lines according to estimates of final consumption and of constructing general purpose sewers triumphed. In the home running water, the WC, the bidet, and the tub or shower with hot water spread more slowly. In laundering, the introduction of detergent and, much later, of the washing machine disrupted time-honored public cleansing rituals. Finally, at the local level, governmental committees were formed to enforce new standards of water purity.

Analyzing newspapers read by different social groups and interviewing aged teachers and doctors, Goubert explores the means by which new standards of water consumption and cleanliness were

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transmitted to the public; he seeks to explain why consumption increased so enormously, even though water was no longer free and, indeed, rose in cost. French demand for pure water quickly surpassed what was strictly necessary for health. To promote water spas, mineral water, soaps, and shampoos, advertisers in both bourgeois and popular newspapers drew on both the scentific and traditional symbolism of water. In rural France water was the agent of magicians and priests. It was associated with healing and with important transitions; complete baths occurred at birth, marriage, and death. Example and social pressure were used to win the public for pure water and personal hygiene. Hospitals and schools played a role in introducing modern toilet facilities to many towns and rural communities. Doctors were among the first to install bathrooms in their homes. More visible to peasants and workers were schoolteachers who were expected to set a personal example of hygiene. In class teachers drove home the point that personal uncleanliness was an object of legitimate disgust.

Goubert sees the introduction of water and the rise of new standards of cleanliness as an aspect of the decline of a traditional world view, based on the fecundity of filth and the cyclical nature of human activity, and as contributing to the rise of a modern view, founded on science and the denial of death. Goubert presents water supply and new standards of cleanliness as more or less interchangeably associated with the modern world and with the bourgeoisie; surprisingly, his discussion of the spread of municipal water supplies ignores the role of socialists and social reformers in democratizing the water supply or in championing the municipalization of water works; cleanliness could be a terrain of conflict as well as a tool of domination.

Still, Goubert's impressive combination of research and analysis is an encyclopedic effort to study the distribution and increased consumption of pure water and to evaluate its historical significance.

MICHAEL HANAGAN
Columbia University

RALPH P. LOCKE. *Music, Musicians, and the Saint-Simonians*. Chicago: University of Chicago Press. 1986. Pp. xvi, 399. Cloth $48.50, paper $21.00.

In their heyday on the Parisian scene, the Saint-Simonians were as remarkable for their costumed processions, chanting, singing, and rituals as for their unorthodox social analysis and theories of reform. In this fine study Ralph P. Locke offers an interdisciplinary view of the Saint-Simonians' aesthetic teachings and their relationships with artists—with musicians and music in particular—and achieves a fresh exposition of the movement in general.

Locke's is the first full-scale treatment of the musical dimension of Saint-Simonianism, and his work integrates considerations as disparate as the master's own unsettled views of the value and place of the art and profession of music, the enhanced valuation given them by his followers under Barthélemy-Prosper Enfantin in the early 1830s, the richly varied relationships of leading European composers with the movement, musical activities among the Enfantian cadre before its diaspora, and the careers of the Saint-Simonian musicians Jules Vinçard, Dominique Tajan-Rogé, and Félicien David. The Saint-Simonians were pioneers in articulating a socially ameliorative role for the arts, and their theories at times embraced both the necessity that the artist (acting as a priest to evoke a religion of concord, of *association*) accept the direction of Enfantin and the propriety of the artist's revolt against authority and the expectations of a "critical" age.

The aura of authoritarianism—and of mystical religion—exuded by the Saint-Simonian movement of the early 1830s was undeniable. Felix Mendelssohn was repelled by the authoritarianism, and Hector Berlioz, by the mysticism. But others, Locke argues, were profoundly influenced by their contacts with the Saint-Simonians: Franz Liszt and Adolphe Nourrit, although they did not formally join, became driven by convictions of the high calling, the seriousness, of the art and of the necessity of ministering to the aesthetic, spiritual needs of ordinary people. David, who, as a whole-hearted convert, gave the movement a composer temperamentally attuned to providing the ritual music for the new religion of concord, found in Saint-Simonianism a home and in Enfantin a true father. Locke contends that the peculiar idiom by which David attained fame, an exoticism nurtured by the sights and sounds of the Near East, owed little to the movement's ideas but was often marshaled in its service.

Such discriminations between the influence of the Saint-Simonians' aesthetic ideas, on the one hand, and the success with which they sought to attract the loyalties of some of the better artists of the day, on the other, are among the felicities of Locke's judicious book.

DAVID WARREN HADLEY
Wake Forest University

JOHN F. SWEETS. *Choices in Vichy France: The French under Nazi Occupation*. New York: Oxford University Press. 1986. Pp. x, 306. $24.95.

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This is a fine piece of work. John F. Sweets knows France and the French well. He is not satisfied with rehashing the stereotypes that have become part of history. Starting with the hypotheses set forth in films such as *Lacombe Lucien* or *Le chagrin et la pitié* (*The Sorrow and the Pity*), Sweets goes on to question works that portray the French during the German occupation as either members of the Resistance from the start or unrepentant collaborators. Sweets has immersed himself in the French and German archives in an effort to draw a rigorous and clear picture of a provincial city, Clermont-Ferrand, and its inhabitants, the refugees who found a place to live there, and its political atmosphere from 1940 to 1944.

With patience and care Sweets analyzes the regimentation of youth, the activities of the French Legion of Veterans, the collaborationist forces, the condition of groups excluded from the new order (Communists, Spanish republicans, Jews), public opinion, and resistance movements. Sweets adds to this vast panorama a most interesting and new chapter on the German view of the city and its problems. This study is rich and wide ranging, filled with information about a specific example, which is, on the whole, not well known.

Sweets's conclusion is well informed and carefully balanced. Most of the French in Clermont-Ferrand were neither collaborators nor members of the Resistance. They were concerned about survival, wrapped up in their daily problems, worried about the fate of "their" prisoners in the stalags and the oflags and, for some of them, about trying to escape from the round-ups that would take them to the concentration camps. It was a time of neither heroic exaltation nor submissive despair. Perhaps their political sympathies were closer to Charles De Gaulle than to Philippe Pétain, unless they saw both men as two facets of the same jewel of patriotism.

Sweets's choice of Clermont-Ferrand is less convincing. Besides the fame brought to it by *Le chagrin et la pitié*, why should this city have been chosen? How does it reveal the general condition of the French during World War II? Readers will be even less convinced of the typicalness of Clermont-Ferrand when they learn that it was transformed by a flood of refugees. Sweets might have offered comparisons with Toulouse, Lyon, or other medium-sized cities. In his preface he talks about local history. So be it. But he does not hide his intentions of making general conclusions from the example of Clermont-Ferrand. From this comes the ambiguous nature of his project and the perplexity of the reader. I fear that Clermont-Ferrand is turning into the tree that hides the forest.

A second criticism concerns the organization of the work, which is thematic rather than chronological. As a result, it misses that 1942 was a major turning point in the history of the occupation. In that year Pierre Laval came back to power, the Allies landed in North Africa, the Catholic clergy protested against the massive arrests of Jews, and so on. This criticism is crucial. I wonder if the atmosphere of 1940 was exactly the same as that of 1941 and if the changes that took place in 1943 have not been lumped together with those of 1944. Things changed quickly and profoundly. That is why the references to dates, now buried in Sweets's treatment of his various themes, deserve a separate place of their own. Chronological organization would even better illuminate the complex and constantly changing situation.

On the whole, this is a book that nobody interested in French history can read with indifference, even if its structure and goal are open to criticism.

ANDRÉ KASPI
The Sorbonne

PHILIP E. CONVERSE and ROY PIERCE. *Political Representation in France*. Cambridge: Belknap Press of Harvard University Press. 1986. Pp. xiii, 996. $49.50.

This is a book by and for political scientists; only historians deeply concerned with the methodology of electoral analysis will wish to work through its 986 densely written pages. Historians seeking a usable synthesis of the working of the postwar French political system will be astounded to find that this massive book covers only the elections to the National Assembly in 1967 and 1968 and, very briefly, the presidential election of 1969. Even this restricted subject, which is approached with great methodological sophistication, unfortunately yields meager findings on the wishes and motivations of French voters and on the responsiveness of deputies to the will of those voters, which Philip E. Converse and Roy Pierce set themselves to analyze.

This inquiry began in 1967 with a well-funded, ambitious sampling of the opinions of 2,046 voters spread through 86 of the 467 electoral districts and of the views of 272 of the 400 candidates running for deputy in those districts. The analysis of these questionnaires forms the main substance of the book. However, the eruption of the student riots and workers' strikes of May–June 1968, which took the authors as well as most professional observers of France by surprise, provoked the dissolution of the National Assembly they had so painstakingly researched. The authors felt compelled to return to carry out new, but more restricted, interviews in 1968 and 1969, primarily to incorporate in their study an interpretation of the response of the newly elected deputies to the students' and workers' demands for reform. As the authors explain, the

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methodological effect was to turn a "multilevel" study of the election of 1967 into a "longitudinal" one as well, for which funding ran out and which required twenty years to finish.

The first of the four parts of the book dissects the French voter, the authors' goal being more to advance the forms of electoral analysis than to reach conclusions of pragmatic significance. For example, a "visibility index" was constructed by asking voters which parties they spontaneously recalled, which they passively recognized, and which they had not heard of. At great risk of unfairly oversimplifying, the authors' main conclusions seem to be that the "voter-party" link in France was loose relative to other democracies, that voters had a clearer sense of their party affiliation than of their position on a "left-right" orientation, and that religion played a greater role in determining a voter's preference than did socioeconomic status.

The second part, on the choice of representatives, concludes that the majority of the French political elite were male, urban, educationally privileged, and from high-status occupations. Even the Communists, though of more modest background, were professional politicians who no longer considered themselves blue- or white-collar workers. Voters were more liable to associate parties than candidates with specific policy issues, perceiving for example the Gaullist Union pour la Nouvelle République as a force for governmental stability and economic development, but knowledge of a particular candidate as a result of his or her "visibility" was of greater importance in influencing their final choice than was party loyalty.

The third part, on the upheaval of May–June 1968, is disappointingly brief and adds little to the conclusions of such concerned observers as Raymond Aron. The authors' conclusion that "on the part of the new parliamentary majority [there was] a certain disposition to reform, touching on the two groups that had been the principal actors May 1968: students and workers . . . But there was little or no disposition among the majority to tolerate fresh outbursts of the same kind" hardly justifies the effort made to reach it.

The last part, on the system of representation, is the heart of the book. Nine long chapters pull together the authors' methodology and place it in the context of previous models of representation developed by political scientists. But once again the findings are hardly surprising for the historian. The authors conclude that the deputies most faithfully represent upper-class or politically active constituents, that in spite of the multiplicity of parties and complexity of the voting procedure the French are no more faithfully represented than are citizens in other democratic systems, and that in fact, because of the complexity of the process of sending and receiving information on voters' wishes, the system "seems to fall short of its potential for much of the electorate much of the time."

Finally, what is one to say about a book on French representation that virtually ignores the presidency as an institution of representation, and, most important, devotes only the briefest space to the overpowering personality of Charles de Gaulle? At times, one has the feeling in reading this book that one is studying the omnipotent National Assembly of the Fourth Republic and not the emasculated body of the early years of the Fifth.

F. ROY WILLIS
University of California, Davis

J. H. ELLIOTT. *The Count-Duke of Olivares: The Statesman in an Age of Decline.* New Haven: Yale University Press. 1986. Pp. xxviii, 733. $29.95.

J. H. Elliott does not describe his long-awaited study of Olivares as a biography. The count-duke dominated Spanish politics from 1622 to 1643 and played a major role in the Thirty Years' War, but most of his personal papers have been destroyed. The author is therefore unwilling to offer a definitive interpretation of his personality. He is also unable to illuminate many of the connections between character and events that are the essence of biography as a genre. This is unfortunate because Olivares was a complex man who was ultimately driven—or who drove himself—beyond the borders of madness. The temptation to speculate on this and other matters must have been great, but Elliott has wisely avoided it. The result is a purely political biography that is also a political history of the age, a magisterial synthesis nearly seven hundred pages long that summarizes a generation of scholarship on seventeenth-century Spain and on much of the rest of Europe as well. The book provides, however, few new or startling interpretations. The author's ideas on the period have been carefully developed in his earlier works and sketched most recently in his *Richelieu and Olivares* (1984). The function of the present work is to integrate these interpretations and to provide a more detailed analysis of the evidence on which they are based.

Elliott sees the count-duke as a minister oppressed by a sense of Spain's decline. An activist by temperament, Olivares attempted to introduce a host of reforms, some of which were revived by the Bourbon monarchs a century later. Many of his schemes seem impractical by modern standards. Others foundered on the shoals of local and class privilege, but the major obstacle to Spain's rejuvenation was the count-duke's own foreign and imperial policy, which aimed above all at preserving his monarch's

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"reputation." Reputation, in this case, involved Spain in endless warfare that it could not afford and thus exacerbated the economic problems bequeathed to Olivares by his predecessors. The regime ended in military stalemate and imperial disintegration with both Catalonia and Portugal in open revolt.

The origins of the count-duke's dilemma are treated skillfully. As the head of a network of clientage, Olivares was at pains to distance himself from the policies of his predecessor, the clever but torpid duke of Lerma. Activism was therefore mandated at home and abroad. In hindsight Lerma's course, though distasteful and perhaps even corrupt, may have been the better choice, for if Lerma had avoided domestic reform he had also sought peace. For Olivares this was politically and temperamentally impossible. The count-duke represented a reforming impulse drawn in part from the *arbitristas*—writers who sought to build a "science of government" on both religious and pragmatic foundations. Some readers may feel that Elliott treats these authors with excessive respect, for if they understood the "ills of Castile" they were too often inclined to see them as symptoms of moral rot. The *rentier* mentality, the neglect of the mechanical arts, and the reluctance to make productive investments were rational economic choices in seventeenth-century Spain. The count-duke's failure to understand this caused him to misjudge his opposition more than once. But if activism caused trouble at home its extension to foreign affairs was worse. Olivares, like most of his contemporaries, believed that only a vigorous military policy could preserve Spain's honor and deter the aggression of others. Although rooted, like all of his ideas, in a devout religious and authoritarian regalism, such a policy imposed burdens too great for the economy to bear and nullified attempts at reform that might otherwise have succeeded.

Obviously, Olivares was carrying a heavy ideological load. He also made mistakes. The Mantuan affair was a disaster and his handling of the Catalans inept, yet Elliott is generous in his final assessment. One of the great virtues of this book is that it never seeks to oversimplify what was at best a difficult situation. It places its readers in the mental world of the seventeenth century and leads them step by step through the labyrinthine difficulties faced by its protagonist. Its analyses of diplomatic problems are as illuminating as its descriptions of court intrigues. At the end it is hard to disagree with Elliott's conclusion that the count-duke was struggling against virtually impossible odds, yet Olivares the man emerges from these pages as both tragic and faintly ridiculous, a statesman who hoped by sheer energy and bombast to transcend the realities of his situation. His failure compounded Spain's difficulties, but, as Elliott asks, could anyone else have done as well? Given the ideas of governance that then prevailed, the answer is probably no.

WILLIAM S. MALTBY
University of Missouri, St. Louis

HEINZ VON ZUR MÜHLEN. *Reval vom 16. bis zum 18. Jahrhundert: Gestalten und Generationen eines Ratsgeschlechts.* (Quellen und Studien zur baltischen Geschichte, number 6.) Vienna: Böhlau. 1985. Pp. xii, 432. DM 128.

In his introduction the author somewhat disarms potential critics by mentioning the dangers of wedding family history to urban history. He is well advised. Such combinations frequently come out neither fish nor fowl. And vanity publications are always suspect. (The book, partly subsidized by the von zur Mühlen family, is clearly a labor of love—for family and for a lost homeland.)

But Heinz von zur Mühlen is not the stereotypical amateur, and his labor of love does not suffer from blindness. For his history of the family from the 1530s to the end of the eighteenth century he uses and carefully cites a great collection of source material from family records and all the major Baltic archives. For the history of Reval he uses the writings of reputable scholars. He leaves great gaps where his material is thin, and, although this leaves the reader somewhat frustrated, it testifies to a commendable honesty. He is always forthright about his speculations, which provide continuity or flesh for bare-bone facts. The guessing is plausible and consonant with context and does not conceal his subjects' faults. Inevitably, the importance of this or that ancestor may be enhanced simply through the attention bestowed on him or her. The author guards himself well without completely resisting the temptation to point out possibilities.

The strictly genealogical information in the book will hardly interest anyone other than descendents of other long-time Estonian families. The well-written descriptions of centuries of Reval's commercial, political, and social affairs will be interesting to a wider readership. And the more detailed discussion of certain von zur Mühlens (or relatives with other names) is occasionally fascinating. Hermen thor Moelen, founder of the "dynasty," who appeared in Reval in the 1530s and who (from Narva) traded in Russian goods, is given special attention. But his offspring, Sophia thor Moelen, and then a long line of Blasiuses, Helmolds, Simons, Pauls, Hermanns, Thomases, Eberhards, and others, as well as their relatives through marriage, partners in business, and allies or rivals in politics are discussed as the materials permit. Sometimes they are allowed

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to speak for themselves through their letters and diaries. Their houses, debts, lawsuits, their offices as burgermasters, elders, or members of the guilds, their triumphs in trade or tragedies in war give color and human dimension to a place and times that otherwise are rapidly fading from our collective memory.

HEINZ E. ELLERSIECK
California Institute of Technology

V. V. DOROSHENKO. *Torgovlia i kupechestvo Rigi v XVII veke* [Trade and the Merchantry of Riga in the Seventeenth Century]. Riga: Zinatne. 1985. Pp. 346. 2 r.

In the seventeenth century the city of Riga was the most important Baltic emporium after Polish Danzig. More significant than Königsberg in both tonnage and value of cargo brought to the port, Riga was the prize in the century-long struggle between Russia, Poland, and Sweden for the possession of Livonia. Whereas Danzig has been the object of intense study in recent years by Polish historians, the Latvian capital has been less favored, although its archival holdings are far more extensive than those of either of its main rivals to the south. V. V. Doroshenko's work is the first full-length study of the commerce of the port in a major European language and, fortunately, is of high quality as well. Spending almost half the text on the essential task of presenting the known data on the flow of goods, ships, and money in and out of the port, the author establishes both the overall scale and fluctuations through time. He then turns to the merchants of Riga and gives a detailed account of the groups within the merchant class and a discussion of the profitability of the merchants' enterprises.

The results will be of considerable interest to scholars of the economic history of the Baltic. Unlike Danzig, Riga was not a major grain port. Hemp, linen, and related products made up the great bulk of the exports, along with some grain and forest products. In return Riga received a great variety of goods, chiefly textiles, salt from Portugal, herring, wines from Southern Europe, and considerable amounts of bullion, for exports were always in excess. Predictably, the Dutch dominated shipping and trade, with the English and Lübeckers following in their wake. The Riga merchants' wealth followed the trade pattern, with the richest being hemp exporters, then linen exporters, and importers last. Doroshenko's analysis of their profits leads to ambiguous results, but they seem to have done less well than their trading partners in Amsterdam or London. The author's previous work in agrarian history has led him to examine the Riga merchants' contacts in the hinterland, and he finds that (in contrast to Reval merchants) the Riga merchants traded mainly with the peasant or small-town merchant, not with the lords. The linen trade was conducted so much with peasants that the Riga merchants who dealt in it were called *Bauernhändler*, and only the small grain exports (about 10 percent or less of the total) and some timber were acquired by purchase from noble landlords.

A useful chapter is devoted to the sources for Riga's economic history, and the literature cited is very full, covering the basic Western works on the Baltic trade in a variety of languages, interwar Latvian historical writings, and German work on Riga, as well as current Polish and Soviet scholarship. Only Latvian émigré historians are missing from his bibliography. Little space is devoted to the policies of the Polish and Swedish governments toward the city and its trade. For historians of the Baltic region's economy who read Russian, Doroshenko has provided a useful and basic study, which is hardly complete, as he stresses, but points in important new directions for future scholars.

PAUL BUSHKOVITCH
Yale University

EDIT RASMUSSEN. *Mester og svend: Studier over københavnske tømrer og murersvendes lønproblemer og sociale forhold 1756–1800* [Master and Journeyman: Studies of the Wage Problems and Social Conditions of Journeymen and Workers in Copenhagen, 1756–1800]. (Skrifter udgivet af Jysk Selskab for Historie, number 43.) Aarhus: Universitets forlaget i Aarhus. 1985. Pp. 343.

In this study of the building industry in eighteenth-century Copenhagen, Edit Rasmussen concentrates on the relationship between master and servant in two trades—masonry and carpentry—and on the relationship between these trades. The position was complicated by a pool of cheap labor in the form of "soldier-journeyman"; men who had served two out of eight years of military service were allowed to enter the labor market but were subject to instant recall, obliged to go on exercises for four weeks in summer, and restricted as to where they could take employment as building workers. Such was the demand for labor at times in Copenhagen that other partly qualified workers—especially Germans from the Hanseatic towns—were also allowed to work alongside members of the local guilds.

Payment of wages was regulated by the magistrates. Traditionally, the building employers charged the client a fixed "wage" for their workers and took a substantial cut from it to cover their own salaries before they handed the rest on to the worker: the less skilled the worker, the bigger the cut. Masons were much better paid than carpenters,

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and summer wages were appreciably higher than winter rates. The carpenters often complained that they could scarcely live from the money provided, particularly in times of inflation, and they sought equality with the masons.

Because of low wages, the seasonal nature of employment, and other reasons, building workers' households often pursued a second occupation—keeping a tavern, taking a lodger, curing fish, dealing in provisions—in which the wife took the main burden of the work. Some of the masons became quite well-off. Many of the carpenters remained extremely poor.

The labor dispute of 1794, the first large organized strike in Denmark, is discussed at length and shown largely to have involved the German workers, though with strong backing from their Danish comrades. The strike inevitably failed, but wages rose afterward as a result of the burning of Copenhagen.

The book maintains a high standard of scholarship. It is also nicely produced and illustrated. Particularly memorable is the contemporary caricature of a Lutheran minister upbraiding the carpenters' strike committee in their own inn in 1794: a better illustration of labor's bored and pointed inattention to the church would be hard to find.

The book includes a twelve-page summary in German.

T. C. SMOUT
University of St. Andrews

MARGOT HÖJFORS HONG. *Ölänningar över haven: Utvandring från Öland 1840–1930; Bakgrund, förlopp, effekter* [Ölanders Overseas: Emigration from Öland 1840–1930; Background, Development, and Consequences]. (Studia Historica Upsaliensia.) Summary in English. Uppsala: Historiska Institutionen vid Uppsala Universitet. 1986. Pp. 276.

Margot Höjfors Hong's study of emigration from the Swedish island of Öland stems from her participation in the seminar on Sweden and America after 1860 at the University of Uppsala. She began work while the seminar was active but finished it following the seminar's end. Her work reflects the influences of Sune Åkerman, Lars-Göran Tedebrand, Hans Norman, and others from the seminar. Her approach is quantitative and based almost entirely on Sweden's excellent demographic sources, especially parish records such as the *husförhörslängder*. She has, however, used "new" sources such as records of postal money orders and the contract records of the emigrant agent Johan Magnus Fogelberg.

This study fills a gap in the literature. The previous standard work on the subject was Helge Nelson's study from 1909. Hong expands Nelson's time frame and uses more extensive sources. After outlining her methodology and introducing pertinent administrative and economic background information about Öland, she discusses the striking demographic development of the island, internal and external migration, migration from six sample parishes singled out for closer study, the transportation options and the work of one emigrant agent, the emigrants' destinations, and the impact of emigration on Öland. A skeletal English summary is included.

The conclusions drawn are extensive. She argues that emigration was an important check on Öland's late nineteenth-century population growth. (Öland's population was 38,000 in 1870 and only 27,295 in 1930.) She maintains that rates of migration varied across the island because of economic differences (in agricultural and industrial development), that migration was male dominated—especially to European destinations, that use of official emigration statistics results in a low total figure because many migrants left the island on work permits instead of emigration permits, that reemigration amounted to ca. 75 percent, that agents such as Fogelberg played an important role in determining rates and destinations of emigration, that Europe was the early destination for most Öland emigrants and that the United States became more important in the late nineteenth century, that the Öland emigrants settled in the United States in a pattern different from the Swedes in general (for example, relatively few settled in the "traditionally" Swedish midwestern areas), that emigrants sent home money that significantly influenced the economy of the island, and that emigration affected marriage patterns, the demographics of age and sex, farm size, and economic development on the island. Overall, she argues that Öland migration was different in a number of ways from the general pattern of Swedish migration.

Quantitative history often is depersonalized and tedious. Hong's book lacks human and individual elements, but it is well written and readable, and it sheds new light on Öland emigration. Her arguments are well stated, carefully documented, and supported by fifty-four tables (with Swedish and English headings), a number of illustrations and charts, and an extensive bibliography. Her work is an excellent addition to the Uppsala seminar's enormous contribution to our knowledge of Swedish population movements in the nineteenth and twentieth centuries.

BYRON J. NORDSTROM
Gustavus Adolphus College

JOHAN VAN DER ZANDE. *Bürger und Beamter: Johann Georg Schlosser 1739–1799*. (Veröffentlichungen des

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Instituts für Europäische Geschichte Mainz, number 119.) Stuttgart: Franz Steiner. 1986. Pp. ix, 219. DM 69.

This is the first scholarly study of the political philosophy of Johann Georg Schlosser to appear in twenty years. Who was this man? One of the more important German anti-Enlightenment *philosophes*, a Frankfurt patrician and syndic, a bourgeois provincial governor in the Margravate of Baden, his memory survived because he was the brother-in-law of Goethe. His rediscovery since World War II by I. Kreienbrink, Detlev Schumann, Helen Liebel, and Klaus Epstein has now culminated in the first book-length study in over a century. Johan van der Zande, as most others, concentrates primarily on Schlosser's moral philosophy and political thought.

Important is van der Zande's attempt to apply recent West German politial science models to pinpoint Schlosser's position among early German conservatives and liberals. Epstein included him with the conservatives, Liebel with the liberals. The author's unfamiliarity with the comparative political evolution of Britain makes it difficult for him to see how very Whiggish Schlosser was and how the French revolution forced him to assert a far more conservative position (as Edmund Burke did) than earlier. Van der Zande, however, interprets Schlosser as a conservative republican and rejects Epstein's thesis that he was a mixed conservative with a "medley" of enlightened thought.

Schlosser's abstractions are far more important for understanding the overall development of German political thought than has previously been understood: anti-Kantian, proto-Hegelian, he was an aristocratic republican imbued with classical learning, the first to translate Aristotle's *Politics* into German, a firm believer in republican values, *Sturm und Drang* writer, a man who hated tyranny, absolutism, and despotism. All political evils could be traced to evil lurking not only in the hearts of despots but also in those of the mob (*Pöbel*). The fading German Reich had preserved its republican virtues in its confederate structure and could avoid revolution only if the privileges of the nobles were watered down, that is, if the educated bourgeois could enjoy equal status in the bureaucracy with the aristocrats of the establishment. Whereas virtue, happiness, and humanity were permanent values, freedom was viewed as a variable independent of the longevity of a nation. Only the wise officials of a *Rechtsstaat* could hope to determine where the general welfare might lie. Certainly, neither enlightened despots nor revolutionary bourgeois could ever hope to define it.

A Freemason, probably also an Illuminate, Schlosser resigned from the Freiburg lodge because he disliked Joseph II. The man attempted to be the conscience of his age, to the annoyance of many of the enlightened princes who ruled Germany. As van der Zande brilliantly makes clear, he was also full of ambiguities, and his brief forays into *Fürstenbund* diplomacy and his efforts to keep Baden neutral in 1792 show him to have followed a cynical policy of mediatization not different from that of the princes and Napoleon, who divided up the old "republican" empire in 1803–06.

The originality of van der Zande's perspective can be seen in his rejection of the classical interpretation of the French revolution as a bourgeois revolution. The bourgeoisie never "seized power." Yet our view of conservative or liberal derives from that assumption. This study makes a major contribution to the history of late eighteenth-century German political thought, a subject little studied in the past. How excellently it depicts the painfully perceived demands for modernization by the Enlightenment! But are Schlosser's agonies really those of the patrician leadership of the declining, soon to be defeated, Holy Roman Empire? The carefully sifted archival materials, especially his letters to Friedrich Jacobi and Johann Jacobi, the Mainz Jacobite Georg Forster, Heinrich Solms, and Johann Gottfried von Herder, would seem to indicate conservative Whiggery could muster less support than the reform bureaucrats of the Stein era. This excellent study includes an almost exhaustive bibliography, which will be of use to students of the era.

HELEN LIEBEL-WECKOWICZ
University of Alberta

HANS-PETER ULLMANN. *Staatsschulden und Reformpolitik: Die Entstehung moderner öffentlicher Schulden in Bayern und Baden 1780–1820.* In two volumes. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 82.) Göttingen: Vandenhoeck und Ruprecht. 1986. Pp. 399; xi, 401–842. DM 176.

This massive work deals with the emergence of modern public debt in Baden and Bavaria between 1780 and 1820. In public finance as in politics and economics, these decades represent a period of transition, from the territorial budgeting of the *ancien régime* to the national fiscal planning of the modern state. Hans-Peter Ullmann divides the work into two parts: the first highlights the main concern of public finance in the late eighteenth century, a debt "explosion"; the second details the solution to this problem, the reform of the management of public debt.

Whereas England, France, and Holland had found new ways of managing their public debt by the early eighteenth century, the German territories continued to use traditional means and methods till

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the century's end. Only the French revolution and Napoleonic regime succeeded in hitching Germany—for two decades—to its politically, socially, and economically more developed western neighbors. Baden and Bavaria, like the rest of the German states, found themselves hard-pressed to modernize, especially in public finance, in which the deficits were mounting under the exigencies of war, territorial redistribution, and social reform. The rise in debts not only defied traditional methods of financing but also caused new administrative, legal, and political problems. This dilemma was eventually resolved through a multilayered process of modernization, implemented in a series of structural innovations that centered on the unification, funding, legalization, centralization, and commercialization of the public debt. Baden and Bavaria thus experienced, at last, the financial revolution that their neighbors had undergone a century before. What was more, the emergence of modern public debt management played a key role in narrowing the gap that still separated the economic development of the German states from that of England, France, and Holland.

Compared to the many studies that deal with private banking, little has been written about public finance in preindustrial Germany. The existing literature dates, in the main, from the turn of the nineteenth century and lacks reliable statistics. Thus, until now, scholars have been unable to draw firm conclusions about the growth of public income and expenditures, the size of yearly budget deficits, or the sources from which these were financed in individual states. The study had to break new ground by drawing, primarily, on unpublished records from the Bayerische Hauptsstaatsarchiv in Munich and the Generallandesarchiv in Karlsruhe. Since these sources document only the public debtor, however, the author also explored the public creditor through the archives of private bankers, in particular those of the Frankfurt Bethmanns.

The work shows how the territorial debts of the *ancien régime* dissolved within four decades and gave way to the national debt of the modern state. In the process, it also defines the characteristics of that national debt and provides a model of its formation. Backed by an exhaustive bibliography and numerous tables, it is, by any measure, a prodigious achievement that fills a major gap in the history of finance. One can only hope that this effort will inspire further studies to illuminate the rest of that "dritte Deutschland" whose economic history has so long been overshadowed by that of Austria and Prussia.

UDO HEYN
California State University, Los Angeles

MARION W. GRAY. *Prussia in Transition: Society and Politics under the Stein Reform Ministry of 1808*. (Transactions of the American Philosophical Society, number 76, part 1.) Philadelphia: The Society. 1986. Pp. 175. $18.00.

Marion W. Gray has published a series of articles that have contributed significantly to our knowledge of the Prussian reform era. His first monograph, a useful study on the work of Freiherr Karl vom Stein, brings together a good deal of this previous material plus much new information. This work is based on an extensive collection of published primary sources and secondary works, as well as selected archival materials located in the Federal Republic. These include the interesting documents on the period collected by Eckart Kehr, a selection of which has subsequently been published. Unfortunately, the author was not granted permission to examine the rich collections located in the archives of the German Democratic Republic.

Nearly one-half of the book is taken up with setting the scene for Stein's thirteen-month ministry. Here Gray provides a well-written review of historiography on Stein and the reform movement; he describes the nature of prereform Prussian society and examines the events that led to the appointment of Stein following Prussia's defeat.

The most valuable part of this work is its detailed analysis of the many proposals and the actual reforms that characterized Stein's brief period in power. Gray describes the organizational changes that were implemented in the government, as well as the first steps toward dealing with the nearly overwhelming financial challenge faced by the state as a result of the Peace of Tilsit. An especially excellent section is the well-informed discussion of the many proposals for the introduction of representative institutions in Prussia at the rural, municipal, provincial, and national levels. He carefully delineates the important connections between political power and the more modern definition of property established by the new arrangements. Finally, he examines the significant changes in the agrarian organization of Prussian society and describes the moderate, evolutionary approach taken by the reformers to the abolition of the guild system. His discussion of the Edict of October 9, 1807, which emancipated the peasants and established more modern arrangements for land ownership, though brief, is nonetheless interesting. He also touches on the serious shortcomings of the reformers' approach to agrarian reform.

The now well known events surrounding Stein's dismissal are briefly described, and Gray concludes the study with a balanced analysis of the achievements and shortcomings of the reform movement. Overall, he argues that the reformers "moved Prus-

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sia toward a free enterprise system and a modern mobile society. Yet [they] were deliberate in failing to undermine institutions, which did not seem at the time to present economic difficulties" (p. 141).

This solid work is the best detailed analysis of Stein's ministry in English, and my observations on it are few. First, Gray analyzes the reforms from an almost exclusively domestic and therefore unnecessarily narrow perspective. The discussion of the social, political, and economic reforms is not sufficiently integrated with the calamitous military and foreign policy events that spawned them. He has not focused on a key goal of the reformers: to create a new state that would be able to generate and mobilize the kind of human energy needed to survive in the political and military reality of an era shaped by the French revolution. Although the author's choice not to focus on the military reforms as a major topic is certainly appropriate, it is nevertheless important to point out that many of the nonmilitary reforms had a significant political and military element. Thus, the reformers repeatedly stated that a purpose of the reforms was to enhance the sense of obligation to, or identification with, the state on the part of emancipated serfs or newly empowered burghers. Certainly, the first and foremost challenge facing Prussia was to regain its military and international political strength. The fascinating and complicated interplay between the domestic reforms and this need to resurrect Prussia as a military and political force in the European arena should have been explained further.

Also, the seemingly contradictory or inconclusive actions sometimes taken by the reformers on a number of important issues can be explained in part by the ambivalent feelings many of the reformers had toward the process of reform itself. As members of a modern and increasingly powerful elite, they were of mixed mind with regard to the diminution of the role of the bureaucracy, and even in some cases of the aristocracy, and the consequent relinquishing of political power they, as officials, had only recently begun to acquire. Some of the peculiar arrangements they proposed for representative institutions bring this out clearly, and there are other examples. This theme, although briefly alluded to, would have profited from much more thorough investigation.

These questions aside, Gray is to be complimented for producing still another valuable addition to the collection of works in English on early nineteenth-century Prussia.

LAWRENCE J. BAACK
Berkeley, California

JACOB KATZ. *The Darker Side of Genius: Richard Wagner's Anti-Semitism.* (Tauber Institute for the Study of European Jewry Series, number 5.) Hanover, N.H.: University Press of New England, for Brandeis University Press, Waltham, Mass. 1986. Pp. xii, 158. $14.95.

Jacob Katz, well known for earlier works on Jewish history, provides this lucid portrait of Wagner's anti-Semitism. Katz's learned analysis of anti-Jewish texts in a survey, *From Prejudice to Destruction: Anti-Semitism, 1700–1933* (1980), includes a chapter on Wagner's role in the history of anti-Semitism. The present volume replicates that study but also examines the place of anti-Semitism in the composer's life. Finding no trace of anti-Semitism in his art, however, Katz refers all musical analysis to music historians, just as he refrains from explaining Wagner's anti-Semitism "from arbitrarily assumed, psychologically conditioned complexes" (p. 120). Instead, he returns to contemporary writings by Wagner and others, on the reading of which he boldly challenges several established assumptions in Wagner literature.

Katz convincingly demonstrates that changes took place in Wagner's attitude toward Jews and Judaism that reflected and reinforced biographical and historical developments. We are reminded, for instance, that nothing in Wagner's letters and other statements preceding his anonymous publication on "Judaism in Music" in 1850 records anti-Jewish sentiments, an absence overshadowed by his increasingly vitriolic attacks after re-publication of the pamphlet under his own name in 1869 and later association with the Third Reich. His earlier "philo-Semitism" actually reflected a relatively tolerant German attitude toward Jews and Jewish gains in civil rights from the 1830s into the early 1870s. The re-publication of the essay in 1869 accorded with the political anti-Semitic movement of the mid-1870s.

A second assumption has been the acceptance that the original goal of the essay was an attack on Judaism; the maligned composer Giacomo Meyerbeer served only to illustrate Jewish disabilities. Katz argues the reverse. To him Wagner used a polemic over Jews in music as a cover for his attack on a rival and published under a pseudonym to hide the personal nature of the affront and not to appear disloyal to a former benefactor. The ensuing controversy, periodic artistic setbacks, and criticism of his art—attributed by Wagner to Jewish opposition to him in reaction to his essay—increased his anti-Jewish ardor, compelled the re-publication, and raised him to the "prototype" of the new anti-Semitism. The rest is history.

This provocative and judicious profile of the darker side of one of Europe's fascinating art-cult figures, though narrowly focused, is steeped in the broad humanist tradition of Wagner exegesis. Familiar with the nuances of a vast historiography,

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moral issues, and current debates concerning Wagner and his symbolic significance, this rigorous historian nonetheless evaluates his subject in terms of Wagner's time.

MICHAEL MEYER
California State University, Northridge

MARTIN WALCHNER. *Entwicklung und Struktur der Tagespresse in Südbaden und Südwürttemberg-Hohenzollern.* Sigmaringen: Jan Thorbecke. 1986. Pp. 211. DM 38.

This study deals with an important theme, the fate of the regional press in modern Germany. Its focus rests on the colorful and distinctive Swabian heartland, from Lake Constance north through the Black Forest to Tübingen and from Ulm across to the Rhine plain, an area renowned for its attachment to hearth, home, and a rather earnest good humor, proud of its mixed and modest economy, its artisans, farmers, and small entrepreneurs. If a local press could have survived anywhere, it might have been here in this staunchly independent part of Germany.

Not so. An area that once supported about one hundred fifty newspapers was by 1983, when Martin Walchner's study ends, controlled by seven publishing units. Newspapers with a circulation under five hundred—rather fondly remembered as *Zwergzeitungen*, dwarf newspapers—disappeared long ago and will no doubt never return. The wars, economic cataclysms, totalitarian rule, and foreign occupation of this century all play a role in this particular German story, as Walchner tells it. Yet the result is of course not particularly German at all; it is in fact identical to the situation in other provincial parts of the West that have been spared the drama so central to the German past. Concentration in the newspaper world appears then to have been highly impersonal and in the end little affected by regional traditions or, for that matter, earthshaking events.

Walchner does not tackle these larger questions, which radiate from his theme and could have made his book of interest to a broad audience. His effort is squarely in the tradition of German *Zeitungswissenschaft*—literally "newspaper science"—an investigative approach that has almost always been characterized by a rather blinkered empiricism. The approach is normally quantitative, the sources are by and large published materials, and the principal concerns are usually the circulation, political orientation, and frequency of publication of papers. Although without question one of the better efforts in the genre, Walchner's study is inevitably largely descriptive and assertive rather than explanatory.

Although his survey begins with the appearance of the first newspapers in the area in the seventeenth century, his best chapters are on the French occupation after 1945. Here he reveals some initiative in research and analysis, using for instance the French archives at Colmar and comparing, though not always convincingly, French occupation policy to that of the Americans and British. The weakest sections are the preceding ones, which consist of little more than a collation of published data. For the Weimar period a mine of information on the newspapers of this region, the private papers of Hermann Dietrich, readily available in Koblenz, has been ignored. Had Walchner consulted these papers he might have been able to fill at least a few of the gaps in his data, but, perhaps more significantly, he might have tempered some of his rather hollow conclusions about the Third Reich, which he views not as the outgrowth of a situation and a culture but as a crude hiatus.

In short, the study will be useful to some as a specialized reference work. It could, however, have been much more.

MODRIS EKSTEINS
Scarborough Campus
University of Toronto

JOST DÜLFFER and KARL HOLL, editors. *Bereit zum Krieg: Kriegsmentalität im wilhelminischen Deutschland 1890–1914; Beiträge zur historischen Friedensforschung.* Göttingen: Vandenhoeck und Ruprecht. 1986. Pp. 230. DM 32.

This collection of ten essays originated in a historical symposium held at Fischerhude near Bremen by the "Arbeitskreis historische Friedensforschung" in 1985. It addresses the question of whether certain right-wing German institutions and pressure groups were prepared to accept the risk of war after the turn of the century and the degree to which they were willing to push for a violent resolution of what they perceived to be Germany's stagnant geopolitical situation under William II. In a way, it is a study of *mentalité*.

Martin Greschat and August-Hermann Leugers argue that, although the official Protestant and Catholic churches sought peace, they embraced the state as an instrument of power and were willing to accept the risk of war in defense of that state. Rüdiger vom Bruch shows that German university professors—especially economists and historians—were in the vanguard of the quest for overseas expansion and were likewise willing, if not eager, to risk armed confrontation to achieve that aim. Not surprisingly, Roger Chickering finds that the pan-Germans lobbied for war as a "release" from the stifling orthodoxy of Wilhelmine Germany, and

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Dieter Düding detects a "social militarization" among the 2.8 million members of the war veterans' associations. With regard to the military, Stig Förster argues that the traditional "conservative" Prussian militarism gave way to a "bourgeois variant" that was much more radical, aggressive, and prepared to accept war in pursuit of global greatness; Detlef Bald depicts an officer corps ever more under the sway of Social Darwinism and technical efficiency. Peter Winzen analyzes Chancellor Bernhard von Bülow's calculus for war during four international crises after 1904 and concludes that Bülow was most prone to draw the sword during the Bosnian crisis of 1908–09. Finally, Wolfgang Mommsen suggests that German public opinion in the decade before 1914 became prepared to accept the risk of war in the face of repeated diplomatic defeats, indeed, that war became almost a self-fulfilling prophecy, or what Mommsen terms the "topos of inevitable war."

By and large, these short essays, especially those by Chickering, Förster, Bald, and Winzen, represent synopses of their recently published monographs. Several authors seek to announce their present or future research with exploratory essays. In the case of Mommsen, we are offered a German version of a piece that he published in 1981 in an anthology on militarism edited by Volker Berghahn and Martin Kitchen. The present collection edited by Jost Dülffer and Karl Holl seems to be useful primarily as an undergraduate reader; as such, an English translation would be valuable. Its major drawback is that, although it offers an incisive look into the attitudes toward war of several right-wing pressure groups and organizations, it offers virtually nothing on the effect that these groups had on official German policy.

HOLGER H. HERWIG
Vanderbilt University

ULRICH LINSE. *Barfüssige Propheten: Erlöser der zwanziger Jahre*. Berlin: Siedler. 1983. Pp. 272.

In February 1921 Ernst Toller, serving a prison sentence imposed for his role in the Munich Soviet Republic of 1919, commented in consternation on the growth of pseudoreligious esoteric societies—all, as he saw them, symptoms of decay. He specifically singled out Leonhard Stark, Karl Thaldorf, and Ludwig (Louis) Haeusser as fathers of such sects.

The prophets of high culture, or "prophets in the cutaway," such as Hermann Keyserling, Walther Rathenau, Oswald Spengler, and Rudolf Steiner, have been safely fitted by scholarship into the fabric of the troubled times after World War I and given the honorable label of "cultural critics" whose concern about the burden of excessive rationality and progressing mechanization of the modern world made them visionaries of a new holism. But now Ulrich Linse has turned to the prophets of popular culture, or "turnip apostles," whose omnipresence alarmed Toller ("croquettes made of turnip green," we might remember, was the principal dish for dinner at Professor Cornelius's inflation-ravaged household in Thomas Mann's "Disorder and Early Sorrow").

Every society has its share of gurus, nudists, occultists, and fanatic vegetarians. But one could argue that since the early nineteenth century the German climate, with the hopes and frustrations stemming from the building of the nation-state as well as from the hothouse industrialization, was singularly conducive to apocalyptic visions defying the realm of "bourgeois" pragmatism and rationality. Certainly, under the impact of war, defeat, inflation, and economic depression, "inflation saints" and other sectarian cultists, who proclaimed the end of the world and salvation, flourished, and the historian has good reason to move them to center stage. No mere marginal figures, they constituted, however awkwardly, an alternative culture in a crisis landscape. The author has diligently gathered materials on them in private collections, church archives, the archive of the German Youth Movement in Burg Ludwigstein, and insane asylums.

It is interesting that Haeusser, the self-appointed "Great Midday," who also fared as "Zarathustra-Haeusser," "Antichrist-Haeusser," and "Rembrandt-Haeusser," in 1920 addressed the Bauhaus community in Weimar for two hours, leaving Walter Gropius, who shook hands with the "master," at least momentarily enchanted. No less interesting is the iconography of the various gurus: the cross, the sword and hammer, the hammer and sickle, the swastika in various designs, and even the star of David. Although it may be a mere miscellaneous fact that the "Haeusser-Bund" actually competed in the May and December Reichstag elections, winning just short of twenty-five thousand and ten thousand votes respectively, it is of some significance that, like the other gurus, Haeusser moved in the orbit of Communism, Anarcho-Syndicalism, National Bolshevism, and National Socialism—all extreme political movements that bordered on the apocalyptic. The "inflation saints," the author rightly concludes, must ultimately be seen as phenomena in a secularized world of pseudoreligiosity of which Hitler's National Socialism was the most profiled manifestation. In 1923 Leonhard Stark wrote in an article "Adolf Hitler or Louis Haeusser?" that, of the two, Hitler was not the spiritual but the political man—out for power not over the self as, so it seems, Haeusser was, but over others. And Hitler was after all the "inflation saint" who became the leader of the millions of Germans hungry for a new gospel. How

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justified were the fears articulated by Toller in his letter of 1921.

This book adds an important dimension to our understanding of the German passion of the twentieth century. Surprisingly, though, the author chose to exclude such Berlin prophets as Joseph ("Cream Cheese") Weissenberg and Erik Jan Hanussen, alias Herschel Steinschneider. There was a connection, too, between Berlin demimonde occultism and the Nazis; Count Helldorf, before he became Berlin police president under Hitler, attended Hanussen's séances but then had him put away immediately after the Nazi seizure of power.

KLEMENS VON KLEMPERER
Smith College

DIETRICH THRÄNHARDT. *Geschichte der Bundesrepublik Deutschland*. (Edition Suhrkamp, new series, number 267.) Frankfurt a/M.: Suhrkamp. 1986. Pp. 271.

Dietrich Thränhardt starts with a simple reminder. The Federal Republic, "provisional" rump state of 1949, has now outlasted the Weimar Republic and the Third Reich together by a decade. It is only a decade shy of the German national state *par excellence*, the Reich founded by Otto von Bismarck. This means that the Bonn republic can no longer be regarded as a single historical phenomenon as earlier analysts from Fritz René Allemann to Alfred Grosser have done. The Bundesrepublik contains within it more than one major period of German history.

In dividing up this history, Thränhardt identifies three major shifts in political culture from the "demo-authoritarian" 1950s to the democratic reforms of the 1960s and 1970s and "governing against crises" since 1974. The key issue in the first two phases is democratization. Thränhardt sees this as a two-step process. German democracy was still tenuous in the 1950s. In fact, the difference between the fourteen years of Weimar and the first fourteen years of the Federal Republic was not that the Federal Republic was more democratic. Both had highly democratic constitutions, but authoritarian traditions remained strong in governments and popular attitudes of both republics. The key difference between Weimar and the early Bonn republic was the Bonn republic's remarkable stability, not only in its government but also in its major labor, business, and religious institutions. This stability permitted Germans to become accustomed to democratic institutions and political behavior. Thränhardt convincingly attributes the stability not so much to prosperity as to antifascism and anticommunism, which successively dominated postwar Germany. Antifascism, coupled with a deep sense of insecurity, drove Germans of the first postwar years to seek safety in unity and cooperation, which in turn led to the creation of the strong, encompassing organizations so typical of postwar Germany. The fear of communism, still accompanied by an urgent need for security, then preserved the stability of the established institutions in the 1950s.

Only after becoming gradually accustomed to democratic institutions was a younger generation of Germans ready to assert its democratic rights in the 1960s. For Thränhardt the key transitional year was 1962, when Willy Brandt began breaking out of West Germany's rigid cold-war confrontation by seeking improvements in Berlin in "small steps" of rapprochement with the East and, more important from Thränhardt's perspective, when the *Spiegel* Affair shook the quasi-authoritarian structures of Konrad Adenauer's government to its foundations. Only then did Germans shed their age-old authoritarian political culture.

Inevitably, the last (and briefest) section on the period since 1974 is more tenuous. Thränhardt believes the general economic crisis and political culture to have been basically continuous since 1974 as the governments of both Helmut Schmidt and Helmut Kohl have tried to cope with the lingering crisis of governability through stopgap measures and retrenchment. Ultimately, the relative success of *Modell Deutschland* may have been more impressive to foreign observers than to Germans. Has Germany finally settled down to the "normal" political life of a Western democracy, which evokes neither horror nor adulation?

DIETHELM PROWE
Carleton College

RONALD LIGHTBOWN. *Mantegna: With a Complete Catalogue of the Paintings, Drawings, and Prints*. Berkeley and Los Angeles: University of California Press. 1986. Pp. 512.

Ronald Lightbown is already well known for his books on Botticelli and on Donatello and Michelozzo. He has now written a long—two hundred fifty double-columned pages—monograph on Mantegna, together with a catalogue of his paintings, drawings, and engravings. The specialized literature on Mantegna is extensive, including recent books on the *Triumphs of Caesar* and the paintings for the *Studiolo* of Isabella d'Este. Nevertheless, Paul Kristeller's monograph of 1901 has remained standard, and, surprisingly, a proper catalogue has not really been attempted before.

Lightbown's book is now the standard reference and will remain so for a long time to come. His text and catalogue are learned, detailed, and always reliable regarding information. No reference has been missed, no document unconsidered, and no

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historical person or event left uninvestigated. All that can fairly be said to be known about Mantegna and his art can be found here, and Lightbown has himself contributed much. His contribution appears not so much through his discovery of new documents as in his systematic integration of vast amounts of information, ranging from technical data concerning Mantegna's works to analysis of existing literary and documentary records—surviving in greater number than generally appreciated—to precise details of costume and court protocol. No one using his book, whether interested in Mantegna in particular, in the history of Mantua in general, or in points of antiquarian detail taken at random, will come away uninstructed.

The shortcomings of the book arise from its virtues, both stemming from the fact that the author is something of a pedant. In consequence the catalogue makes for easier reading than the text, in which broad themes become lost in tangential digressions. Extended and often tedious description substitutes for stylistic analysis, and when Lightbown touches on matters of stylistic expression he can be very wide of the mark. He complains several times, for example, of an emptiness and insincerity of sentiment in Mantegna's later paintings, which he ascribes to what Michelangelo and Vasari attacked as the clumsy "devout style." But they were attacking the expression of pious sentiment in Flemish art and in the paintings of Perugino and Francia, something quite different from Mantegna's rather dusty Roman austerity. Lightbown's discussion of iconography similarly suffers from an additive enumeration of texts unregulated by analytic method, sometimes producing startling results. Thus, we learn that Mantegna, in his painting entitled *Parnassus*, represented Mount Parnassus in the Vale of Tempe, a geographical absurdity. Or again, we are told that in his *Pallas expelling the Vices* "the tree of Daphne [is] figured as an olive, the tree of Pallas, not as a laurel, the tree of Apollo" (p. 202), both a botanical absurdity and a linguistic blunder, *daphne* being the Greek word for laurel.

Lightbown's strengths, and they are formidable, are as compiler and as a social historian. His weaknesses, equally formidable, are as an interpreter of style and as a historian of humanism. The implications of the samples given above suffice to show why. Despite these drawbacks, his achievement is impressive, and all scholars stand greatly in his debt.

CHARLES DEMPSEY
Johns Hopkins University

TORGIL MAGNUSON. *Rome in the Age of Bernini*. Volume 2, *From the Election of Innocent X to the Death of Innocent XI*. Atlantic Highlands, N.J.: Humanities or Almqvist and Wiksell, Stockholm. 1986. Pp. viii, 404.

This is the second, concluding volume of a survey of the art of baroque Rome. Torgil Magnuson begins each of the three sections of the book with an evaluation of the relevant pontiff and the chief political and religious issues of the day. He then studies the architecture, sculpture, spectacles, painting, and music, the last to a limited degree.

The volume begins with Innocent X (Pamphili), 1644–55. A cautious old man with conservative tastes, the pope nevertheless took a personal interest in, and made final decisions on, several major projects. For example, instead of giving Francesco Borromini a free hand on the rebuilding of the Basilica of St. John Lateran, he insisted on preserving the medieval sections, with mixed results. A certain amount of architecture by committee ensued. Because Innocent did not favor Bernini, the latter devoted himself to private commissions including the "Ecstasy of St. Theresa," perhaps the greatest work of the pontificate. The Piazza Navona also enjoyed a spectacular artistic development at this time. The foreigners Nicolas Poussin and Claude Lorrain dominated painting, and the Italian *bamboccianti*, who painted scenes of everyday life, also contributed. The *bamboccianti* obviously could not obtain grand papal commissions but earned good livings without them.

Magnuson devotes the middle third of his book to the pontificate of Alexander VII (1655–67), who shared the spotlight with Queen Christina of Sweden, the most flamboyant royal convert in many a century. Back in favor, Bernini created some of his greatest works, such as the colonnade of St. Peter's. Alexander's warm, enthusiastic patronage of the theatrical Bernini went hand-in-hand with the pope's condemnation of dour Jansenism. Magnuson provides a good panorama of this epoch, including the transient spectacle art for the celebrations that Romans loved. Indeed, Bernini's studio also designed and prepared some of the elaborate pastries set before Queen Christina. Even the forty-eight-hours' devotion became the occasion for extravagant displays replete with fireworks.

Alexander VII's death closed this great epoch in Rome's artistic and cultural life. It effectively marked the end of the era of Bernini, even though the master lived to 1680. The last section of the book describes a recession through three pontificates, 1667 through 1689. A lack of wealthy, discerning patrons, a changed religious and cultural atmosphere, and papal financial problems too severe to ignore any longer brought the great papal commissions to an end. The book finishes with two indexes (persons followed by places and subjects) for both volumes. It has numerous well-placed black-and-

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white illustrations, ample notes, but no bibliography.

I praised volume 1 (see *The International History Review* 5 [1983]: 450–51) and am happy to repeat my judgment. This is an excellent book. Magnuson provides a judicious historical survey of Roman art, supported by years of reading and looking. This should become the standard survey of Roman baroque art.

PAUL F. GRENDLER
University of Toronto

FIORELLA BARTOCCINI. *Roma nell'ottocento: Il tramonto della "Città Santa" nascita di una capitale.* (Storia di Roma, number 16.) Bologna: Cappelli. 1985. Pp. 872.

This sixteenth volume published by the National Institute for Roman Studies concentrates on the history of the Eternal City in the nineteenth century. This important period, which witnessed the eclipse of papal Rome and its dawn as the capital of the Kingdom of Italy, has not been neglected. A massive literature—historical, descriptive, and nostalgic—deals with the political, artistic, economic, and social life of nineteenth-century Rome. A number of these works are broad in scope, such as M. Birindelli's *Roma italiana: Come fare una capitale e disfare una città* (1978), whereas others are specific, such as Anna Maria Isastia's *Roma nel 1859* (1978), which focuses on one year in the life of the city. Some indication of this vast literature can be gleaned from the critical biographical note at the end of the present volume and from the volume collected on the occasion of the centenary of the Italian entry, *Roma capitale: Bibliografia del centenario* (1971). Understandably, many of these works are in Italian, but foreigners in Rome have left interesting accounts of their years there, and a number of studies have been published in English. Raffaele De Cesare's classic social history, which uses a host of anecdotes and oral reports, has been translated into English under the title *The Last Days of Papal Rome, 1850–1870* (1909). In 1970 Glorney Bolton produced a more popular if broader picture in his *Roman Century, 1870–1970*, which the author appropriately subtitled *A Portrait of Rome as the Capital of Italy*. Recently, Christopher Hibbert published *Rome: The Biography of a City* (1985), which surveys events in the city from its beginning to the assassination of Aldo Moro in 1978.

This is not the first time that Fiorella Bartoccini, a specialist on the Risorgimento has turned her attention to nineteenth-century Rome. In her *La "Roma dei Romani"* (1971) she sought to explain why the Romans proved unable to determine their own political destiny and required outside intervention for inclusion within the unitary kingdom. The conclusions she reached about the immaturity, isolation, and political passivity of the Romans in her earlier work are repeated in the present volume. She depicts the city, surrounded by a sparsely settled malarial plain, as physically, psychologically, and politically isolated from the rest of the peninsula. Moreover, she suggests that the city's inhabitants were nonetheless spared the pains of progress and were in many ways satisfied with the institutions and way of life—in large measure shaped by the papacy. The author also claims that, although the revolutionary and Napoleonic period awoke the Romans from their long slumber, the brief French occupation and the revolutionary and republican years failed to transform the structure of society, prevailing opinions, political institutions, and the pace of life. In her view the nationalist movement proved a more powerful stimulus for change, but the revolution of 1848 was followed by the restoration of 1849, and Rome's social and political life resumed its traditional pace. Only after 1870 was the Eternal City profoundly altered, although Bartoccini stresses that the old population interacted with the new in affecting a synthesis by which the old residents were Italianized and the new were Romanized.

The volume is divided into two parts. The first half, which includes nine chapters, examines developments in Rome from the French intrusion to the Italian arrival in 1870. The second half, which likewise has nine chapters, treats developments during the first three decades of Rome's existence as capital of Italy. Based on a wide variety of primary sources and secondary studies, the book examines political, cultural, social, and economic developments. Unfortunately, these subjects are studied in a confused manner, with scant reference to chronology and redundancy that flows from the organization of the work. Clarity is not the strong point of this study. Indeed, the pages of chapter 3 of part 1 that have been placed out of sequence by the printer do not read much worse than the rest of the narrative!

Despite the poor organization, confused chronology, and constant repetition, these pages contain valuable information about Rome and its population during the nineteenth century. The economic and demographic data are particularly useful and will be welcomed by the specialist. The broader generalizations and profound observations about the people of Rome will provide insights for those interested in the Roman question and the impact of the city on Liberal foreign policy. Most will not care or need to read the work from cover to cover but will dip into it for specific facts. The index of names will prove useful in this respect. The inclusion of a

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subject index would have further facilitated the use of this rambling, though valuable, study.

FRANK J. COPPA
St. John's University

HERMANN WIESFLECKER. *Kaiser Maximilian I: Das Reich, Österreich und Europa an der Wende zur Neuzeit.* Volume 5, *Der Kaiser und seine Umwelt: Hof, Staat, Wirtschaft, Gesellschaft und Kultur.* Munich: R. Oldenbourg. 1986. Pp. xxxi, 904. DM 180.

In this volume Hermann Wiesflecker completes his monumental study of Maximilian I, an endeavor stretching over more than a quarter-century. The first volume, published in 1971, surveys the historical traditions and interpretations; the subsequent three volumes deal in detail with Maximilian's career chronologically. This concluding part promises to pull together the main themes previously dealt with piecemeal. Inevitably, this involves a great deal of repetition of both fact and interpretation; in some sections the majority of notes refer back to previous volumes. Unfortunately, the fragmented organization of the summary produces yet more repetition.

Those familiar with Wiesflecker's earlier volumes will find few surprises in this one. To the extent he has real arguments with his predecessors, they are mostly battles with the *Kleindeutsch* nationalists of the nineteenth century, Leopold von Ranke, Heinrich Ulmann, and Heinrich von Sybel in particular, who saw in Maximilian a dynast sacrificing the "German Nation" to Habsburg imperial policy. Wiesflecker counters with the argument that Maximilian created the Habsburg monarchy, which later preserved the empire from the Turks and from France, almost entirely with resources drawn from his *Hausmacht*, particularly the Austrian lands, which contributed about ten times as much as all the rest of the German states together. Maximilian's intent was always to restore the Roman empire now translated to Germany, a vision that Wiesflecker agrees was directly contrary to the contemporary tendency toward consolidation at the national or regional level. Although this interpretation may be oversimplified, it helps make the obvious contradictions between intent and achievement, as well as the more extravagant ambiguities of Maximilian's personality, more comprehensible to a modern observer.

This is old-fashioned positivist history: the author even cites Ranke's "wie es eigentlich gewesen ist" as his own credo (p. xxxii). In a curious way the book resembles the elaborately drawn *Ehrenpforte* that Maximilian displayed to those attending the Vienna Congress in 1515, a vast schematic arch surrounding a family tree, with innumerable sketches "of the emperor, his family, his domains, his great deeds and small ones, but disguised in numerous allegories and symbols, so that discovering the meaning requires penetrating study" (p. 369). One cannot doubt that every verifiable fact about Maximilian I is to be found at least once in these pages, often many times. Wiesflecker makes, unfortunately, little distinction between what is known and what is important.

The material amassed in these five thick volumes is unquestionably impressive, but it remains in many ways still raw material awaiting further refinement. The notes constitute a good quarter of the book. Neither the bibliography nor the index is cumulative, so readers searching for a specific topic will have to consult each volume separately.

JOHN P. SPIELMAN
Haverford College

F. GUNTHER EYCK. *Loyal Rebels: Andreas Hofer and the Tyrolean Uprising of 1809.* Lanham, Md.: University Press of America. 1986. Pp. xvii, 278. Cloth $26.75, paper $14.75.

F. Gunther Eyck states at the outset that Tyrol's refusal to accept its place in Napoleon's new European political order stemmed from the ill-advised "interference threatening [Tyrolese] customs, institutions, liberties and beliefs" of France's Bavarian satellite (p. 1). Eyck does not, however, view the uprising as an example of opposition to modernization, which the extraneous attempts at fiscal, administrative, and socioreligious reform almost certainly represented. Rather, he seems to regard the rebellion as a manifestation of geographic determinism—a reflection of the mountainous character of the land. This perhaps outmoded conceptualization is paralleled by the descriptive, rather than analytical, focus of the book.

Although Eyck has exploited a vast range of literature and has exhaustively, indeed excessively, annotated his presentation (at least in his background chapter, which treats what contemporary Austrian historians would call Tyrol's *Landwerdung* and the "old corporative society"), he has not provided up-to-date information and interpretations. The catch-all category of "peasants" does not adequately characterize Tyrol's social structure. To be sure, Eyck is aware of internal stratification, since he stresses the important communal functions of innkeepers such as Hofer and mentions social tensions in the larger towns and cities.

The volume is marred by awkward prose, which even the revision noted in the acknowledgments has not ameliorated. The text will try the patience even of readers fascinated by the topic; one sorely misses the hand of an adept editor. Another flaw is the dearth of maps. A cartographic depiction of military

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movements would have strengthened the study, which includes only one general map.

Nevertheless, the book is welcome and useful. Eyck does scholars who do not read German a real service by sketching the immediate origins of the conflict, the see-saw course of combat, and the actions of individual protagonists. He sharply delineates the personality of Hofer, who, for all his attractive features, was hardly suited for the responsibilities thrust on him. Semiliterate and basically innocent of the larger world, he lacked the vision to lead his homeland effectively, especially after Napoleon defeated the main Austrian army at Wagram and made further resistance futile. Hofer's historical prominence probably results from his martyrdom and posthumous role as the symbol of Tyrol's powerful regional-ethnic identity.

Another of the book's strengths is its portrayal of the Tyrolese militia system. Eyck points to the relationship between the widespread, economically significant activity of hunting and the efficacy of elite companies of rifle-toting marksmen. The insurgents, however, lacked professional military attitudes (especially consciousness of the need for subordination) and an effective command apparatus. Further, the book makes clear that most of the engagements between the Tyrolese citizen-soldiers and their Bavaro-French adversaries were not examples of hit-and-run guerilla warfare. However undisciplined and badly led and equipped (save for their fine rifles), the rebels held defensive lines and fought pitched battles.

Eyck effectively surveys other historians' interpretation of the events and Hofer's historical image. The author emphasizes that the struggle should not be seen as an old-fashioned *Bauernkrieg* or jacquerie; the insurgents were not primarily protesting exploitation by manorial lords. Further, Eyck demonstrates that German nationalist historians exploited Hofer's memory. The last section of the book is a handy survey of conditions in South Tyrol since Italy's assumption of sovereignty in 1919. In this context Hofer's figure continues to be symbolically crucial.

Its defects notwithstanding, Eyck's book will remain a worthwhile reference tool. Students of ethnic minorities, war, and society (especially those interested in the comparative study of militia forces) will surely profit from this book.

THOMAS M. BARKER
State University of New York, Albany

EDWARD ACTON. *Russia*. (The Present and the Past.) New York: Longman, 1986. Pp. xiii, 342. Cloth $29.95, paper $14.95.

Part of a series providing background information on major countries, this slender volume aims to provide a historical survey of Russia from its beginnings to the present to enable laypeople to understand the contemporary role of the Soviet Union. Edward Acton's expertise lies in nineteenth-century intellectual history. At the outset Acton states that he will focus on five vital themes: how Moscow came to rule the vast Russian empire; how Russia's international position interacted with its sociopolitical development; why such a radical revolution erupted in the most backward European great power; the impact of this backwardness on post-1917 developments; and how the state has come in the Soviet Union to regulate such a broad range of human activity. "It is the relationship between State and society . . . which provides the guiding thread of my analysis" (p. xiii).

I was struck by the gross neglect of Russia's early history in this book. Only one-fifth of the whole deals with the first thousand years of Russia's development as a nation (ca. 860–1855), far too little even to outline major themes, much less to present a coherent narrative. Acton's presentation of early Russian history is as likely to confuse as to enlighten the reader. One finds statements such as: "Meanwhile, resistance to foreign overlordship was stiffened by merchant mercenaries from Scandinavia" (p. 3). Or try this gem: "Major urban riots in 1648, however, concentrated the government's mind" (p. 30). References such as "the Golden Horde of Sarai" (p. 10) and the "State of Kiev" (p. 3) may mislead the untutored. Downright incorrect are the assertions that when Ivan III renounced Mongol suzerainty, "they [the Mongols] were unable to strike back" (p. 13) and that Moscow became the center of a separate principality in 1301 (p. 10). (Normally, the year 1263 is cited for that event.) Dmitrii Ivanovich, who died mysteriously in Uglich in 1591, was Ivan IV's son, not grandson (p. 15).

On the other hand, chapters 4 through 12 present a sound if rather conventional and strictly chronological survey of post-1855 Russian and Soviet history. Coverage of political, economic, and social developments after 1855 is generally clear, accurate, and complete. Notably full is the treatment of the revolutions of 1917, although the triumph of Bolshevism is not really explained. Grievously neglected, however, are the role of the Orthodox church, women, nationalities, tsarist foreign policy, Marxism, and cultural development. Therefore, this volume scarcely achieves its professed goal of providing the lay reader with a balanced view of Russia's development. Perhaps such a task is impossible of achievement in three hundred pages.

On the whole Acton, after a judicious treatment of the horrendous crimes of the Stalin era, assesses the post-Stalin regimes of Nikita Khrushchev and

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Leonid Brezhnev rather favorably. Acton emphasizes the great improvement since 1953 in political stability, popular support for the Soviet regime, and living standards. Twelve good maps elucidate the text; there are many illustrations, although they are uneven in quality. In a bibliographical essay the author includes and evaluates key sources for each chapter, including some in Russian. Almost no books on tsarist foreign policy or the role of women are cited. Explanatory subheadings for each chapter would have been helpful. Despite numerous flaws, this volume, targeted for the lay reader and "history buff," will introduce them to some elements of Russia's modern historical evolution.

DAVID MACKENZIE
University of North Carolina, Greensboro

MICHAEL F. HAMM, editor. *The City in Late Imperial Russia*. (Indiana-Michigan Series in Russian and East European Studies.) Bloomington: Indiana University Press. 1986. Pp. viii, 372. $27.50.

In 1976 Michael F. Hamm edited a useful collection of articles on the Russian city entitled *The City in Russian History*. In his introduction to that volume he accurately described it as "the first attempt outside of the Soviet Union to examine the Russian town through time." Notwithstanding its many virtues, the volume suffered from a certain lack of cohesion, probably the inevitable consequence of attempting to squeeze a thousand years between two covers and using sixteen authors from several disciplines.

The collection under review (in a loose sense a sequel to the earlier volume) does not suffer from that problem. Indeed, it is one of the most coherent and unified collaborative works in the field of Russian history. Each of the eight substantive chapters examines a single city (Moscow, St. Petersburg, Kiev, Warsaw, Riga, Odessa, Tiflis, Baku), covers roughly the same period (1861 to the early twentieth century, with some attention to the pre-1861 background), and, most important, addresses more or less the same problems. The essays cover demographic growth and change, economic development, the transformation of the urban environment (sometimes referred to as the "social fabric"), animosities between ethnic groups (except in Moscow and St. Petersburg, where the issue was largely irrelevant) and the relative ability of the elected city council (*duma*)—introduced as part of the Municipal Reform of 1870—to cope with these problems when faced with the dual challenge of a suspicious and overweaning bureaucracy and a constituency sharply divided along class and ethnic lines. Moreover, in contrast to the previous collection, all but one of the contributors are historians, and the sole exception, James Bater, is a historical geographer. Happily, despite this unified structure, the individual chapters are not mechanically reproduced clones. The authors cover their respective cities with a unique touch and order the sequence of their presentations to conform with the peculiar character of the particular city. The question of ethnicity—so heavily stressed in most chapters that this emphasis probably should have been reflected in the book's title—is handled differently in each study, in part because of the authors' diverse linguistic skills, but mainly in recognition of the cities' diverse ethnic histories.

In an excellent concluding chapter on urban revolution, Daniel Brower, the author of a forthcoming book on urbanization in Russia, attempts to summarize the patterns of change revealed in the preceding chapters and emphasizes their implications for the explosive rise of revolutionary political action after the turn of the century. The most important generalizations that emerge may be summed up as follows. Industrialization per se was less influential than general economic, commercial, and demographic growth in determining the character of the late imperial city. The coexistence of modern capitalist elements with a traditional, small-scale, essentially artisanal and even peasant economy (astutely described as a "dual economy" in Joseph Bradley's fine chapter on Moscow) was one of the city's salient features. In the six "borderland" cities, ethnic self-consciousness was stimulated rather than stifled by urban life (not many melting pots here!), creating enormous difficulties for the government but simultaneously thwarting the efforts of opponents of autocracy to build a united movement. (Ronald Suny's chapter on Tiflis is particularly illuminating on these matters.) Municipal dumas made some valiant efforts to halt the deterioration of the urban social fabric, but on balance theirs was a story of failure, especially from the point of view of the lower-class denizens of the cities' outlying areas. (Brower is somewhat more optimistic on this point, at least in tone, than other contributors.) Finally, ethnic divisions notwithstanding, the urban milieu was conducive to some limited successes for the revolutionary movement (especially the revolutionary labor movement), however short-lived and weakened by ethnic strife those successes proved to be.

So brief a summary cannot do justice to the richness of the data and the complexity of the arguments presented by the talented authors of this valuable volume. Only a careful reading of the book can demonstrate the contributors' nuanced presentation and interlacing of the patterns noted above. My only regrets are the absence of a more elaborate comparison between the Russian city and its West European counterparts (the contributors make mul-

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tiple allusions to Europe but provide little systematic analysis) and the editor's decision, weakly rationalized in the introduction, to deemphasize the events of 1905. One yearns to see the ways in which the developments so lucidly analyzed in this volume took on day-to-day political significance at a moment of revolutionary crisis.

REGINALD E. ZELNIK
University of California, Berkeley

K. F. SHATSILLO. *Russkii liberalizm nakanune revoliutsii 1905–1907 gg.: Organizatsiia, programmy, taktika* [Russian Liberalism on the Eve of the 1905 Revolution: Organization, Programs, Tactics]. Moscow: Nauka. 1985. Pp. 346. 3 r.

K. F. Shatsillo's study of the programmatic and tactical differences among Russian liberals from 1902 to 1905 is based largely on unpublished materials in Soviet archives, and they constitute the book's major interest. Increasingly, both Western and Soviet historians have been asking not when the liberals' dream of a peaceful transition from autocracy to representative government became impossible to realize but whether, in the Russian context, it was ever realistic. Terence Emmons and William Rosenberg, discussing the Kadets in the 1905 revolution and the liberals in 1917, advance much the same conclusion as Shatsillo for this earlier period: that liberalism's twin dilemmas, namely, its lack of a sizeable natural constituency and its reluctance to use radical means to promote its goals, promoted terminal paralysis.

Shatsillo's access to Communist party archives gives him an advantage over Western historians (whom he criticizes, Emmons excepted, for insufficient archival research). In his second chapter he quotes extensively from correspondence between the collaborators on the journal *Osvobozhdenie*, edited by Peter Struve, and cites numerous unflattering or mistrustful remarks about Struve, which, in Shatsillo's opinion, negate Richard Pipes's positive assessment of Struve's ideas and role. The third and fourth chapters similarly focus on the destructive quarrels within the liberals' organizations and their consequences for the cause.

Without doubt, the liberals' besetting problem was to create a program and tactical strategies appropriate to a spectrum of reform-minded people whose attitudes ranged from neo-Slavophile to socialist. Because they could not unite these groups, at critical moments this loose coalition shattered, the left going over to the masses, and the right to the autocracy. The problem for the modern historian is how to evaluate their efforts. Emmons discusses the coalition dilemma sympathetically and avoids its complications by focusing on the constitutionalist center. Shatsillo's solution is to approach the liberal spectrum from the Marxist perspective that he insists is superior to the amorphous Western definition of liberalism. Thus, in this otherwise illuminating study, he tends *a priori* to dismiss many liberal ideas as myopic, engendered by personal weakness, not ideological conviction. For example, Shatsillo considers the liberals' insistence that their unions were "above-class" or "nonclass" a self-deceptive, if not deliberate, attempt to mislead others. The same is true of the reluctance of some liberals, in the heat of 1905, to use revolutionary tactics that were possibly attractive to the mass constituency they needed but compromising to their ultimate goal of legalism. The book closes with S. L. Frank's confession (extracted from a letter in the party archives) to Struve in the spring of 1905: "No matter how much I long for political liberty, I can neither kill people for it, nor summon them to death, nor—speaking openly—myself become cannon fodder" (p. 326). Without seeing the context, who can say whether Frank was being principled or pusillanimous? Although Shatsillo's interpretation remains open to question, such citations make his book an important source of new information.

PRISCILLA R. ROOSEVELT
Catholic University of America

ALEX DE JONGE. *Stalin and the Shaping of the Soviet Union*. New York: William Morrow. 1986. Pp. 542. $19.95.

Perhaps there is a kind of justice in the fact that the great demonic figures of history ultimately fall into the hands of popular biographers. Alex de Jonge, the author of lives of Peter the Great and Rasputin, has taken Joseph Stalin as his next subject, and his demolition of any lingering enthusiasm for the "man of steel" is nearly complete. Written in the lively, novelistic style appropriate to books designed for a large market, this book makes no great demands on its audience. Rather, the author chronologically leads the reader through this eventful period and punctuates the text with moral pronouncements, offhanded aesthetic judgments, and ironic previews of what is to come. ("He may have refrained from criticizing Kamenev in 1915, but he shot him in 1936 just the same" [p. 91].) De Jonge textures his account with appropriate anecdotes, contemporary jokes, and excerpts from memoirs that give the story a feel for life in the Stalin years. The writing is fluid, the analysis superficial, and the focus squarely on the personality of Stalin himself.

This is neither a work of original scholarship nor a reinterpretation that leads to new understanding. Rather, the complexities of Russia's social environment and the Social Democratic movement in which

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Stalin attained his political maturity are simply smoothed away to leave an unproblematic narrative of a rather ordinary man in extraordinary circumstances. Since the unsurprising thesis of the book concerns Stalin's drive for personal power, the political and social worlds in which he operated are treated as less relevant than his psychological proclivities. One looks in vain here for the meaning of the prerevolutionary debates between Mensheviks and Bolsheviks, analysis of the conflicts in the 1920s on developmental strategies, or a sense of the political confusion that accompanied the frantic construction and destruction of the 1930s. None of the difficult issues of Soviet historiography are faced and dealt with. No attempt is made to use the extensive monographic literature of the last decades that has significantly reshaped the picture we have of late imperial and early Soviet Russia. Although dissident memoirs, Western secondary literature, and material from British archives are used, nothing is really added to the best of the extant biographies of Stalin, those of Isaac Deutscher and Robert C. Tucker.

The problem of Stalin, perhaps the central issue of postrevolutionary Soviet history, remains so profound in its implications for the system he built that it cannot yet be faced by Soviet officialdom. Whether one seeks its origins in the fatal continuities of Russia's history or the innovative impact of Leninist ideology, the conclusion is unavoidable that socialism in the Soviet Union and elsewhere has been indelibly tainted by the Stalinist experience. For de Jonge, however, Stalinism is deceptively simple. The result of neither inevitability nor necessity ("questions that have no meaning beyond the Marxist framework"), the horrors of Stalinism are less a product of a specific historical experience than the working out of the logic of socialism. ("The development of societies favoring Western democratic institutions and individual rights is inextricably bound up with the emergence of unconditional property tenure; each informs the other. The essence of Stalin's system and its attitude to the rights of individuals derive from a totally opposing conception of the way in which property should be held" [p. 490].) Despite all the space taken up with the person of the dictator, the concluding theme of the book is that something quite like Stalinism would have developed in Russia without the man himself. "Russia could never have become a superpower without a coercive regime. It was a Grain Belt culture, its manpower unsuited to industrial discipline . . . Russians are free spirits, incapable of creating their own work ethic" (p. 491). Such a grand thesis is nowhere defended or adequately demonstrated in the body of the work.

With the recent interest in Soviet social history Stalin's "revolution" has become a center of controversy in both scholarly and wider political debates. For the Right it serves as a warning against the attraction of revolution and utopian thinking and reinforces the acceptance of capitalism. For the Left it poses the most difficult questions of the power of existing social structures, the weight of backwardness and custom, and the limits of using force to create a preferred reality. For revisionist scholars the old models of totalitarianism and the monolithic police state are no longer adequate, yet new syntheses based on the limited empirical work have still to be made. Although de Jonge provides some interesting details on this traumatic and still poorly understood phenomenon, a deeper understanding of the contours of Stalinism and the dynamics of the life of its founder will have to await the results of work now underway.

RONALD GRIGOR SUNY
University of Michigan

ROBERT CONQUEST. *The Harvest of Sorrow: Soviet Collectivization and the Terror-Famine*. New York: Oxford University Press. 1986. Pp. 412. $19.95.

This is the most comprehensive history of the Soviet agricultural crisis that, more than one-half century later, continues to affect the output of food in the Soviet Union. Robert Conquest's account is also the most vivid portrayal of one of the great crimes against humanity of the twentieth century. Deservedly, the book will serve as a basic reference for more general histories of the Soviet system. Therefore, a clear assessment of its strengths and weaknesses is important.

Conquest is strongest in his analysis of Leninist ideology and its changing impact on society. After a brief discussion of the peasantry under the tsars, he points out that Lenin accepted Marx's assumption that "the distinction between town and country" had to be obliterated by "establishment of industrial armies especially for agriculture." For early Bolsheviks this was a hard directive, for (contrary to Marx's expectations) they had attained power in an overwhelmingly peasant country. But, apparently, the danger of submerging radical social transformation in a sea of recalcitrant peasants convinced Lenin to rely on urban strongholds as soon as "the initial phase of alliance with the peasantry as a whole was over and . . . the socialist revolution could now begin in earnest" (p. 45). For four years following Lenin's death this "as soon as" had to be put off for tactical reasons, but Conquest makes a convincing case that even a "rightist" such as Nikolai Bukharin was unreconciled to a permanently autonomous peasantry. Conquest's detailed evidence bears out Herbert J. Ellison's conclusion (*Slavic Review* 20 [1961]: 197) that Soviet leaders deciding on collectivization of agriculture were really more concerned with the

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political than the economic viability of their alternatives. Moreover, Conquest's massive evidence on the process of collectivization demonstrates that party activists in the field were also convinced that the central issue was subduing the independent peasantry regardless of heavy costs, rather than obtaining purely economic benefits.

Conquest's discussion is complicated by his effort to assess the varying geographic and ethnic impact of collectivization. In fact, his evidence shows that the impact was very broad. The proportion of human lives lost, to say nothing of livestock, was greatest among nomadic peoples, notably Kazakhs. In Europe repression of "kulak" families and (three years later) starvation of other peasants was frightful throughout the Volga (German and Russian), the Don (Russian Cossack), and the North Caucasus (Ukrainian and Russian) black-earth regions as well as the Ukraine. Even a grey-earth province such as Smolensk provides (because of the accident of archival availability) much evidence of ruthless treatment of peasants, regardless of personal "guilt." Within the Ukraine itself, Conquest notes, there was a sharp gradation of loss from the forested, grey-earth northern provinces to the black-earth steppe nearer the Black Sea. His interpretation of this phenomenon might have been sharpened by consideration of the special circumstances of the *lesostep* intermediate agricultural zone, mainly on the right bank of the Dnieper River. Consideration of the large Russian and German peasant minorities in the decimated Black Sea provinces might have led him to modify his contention that non-Ukrainian deaths constituted only one-seventh of the total in the Ukraine and North Caucasus. Nevertheless, although the causal connection is hard to prove, Conquest is almost surely right in emphasizing the coincidence between Stalin's assault on Ukrainian peasants, the largest and most compact of the black-earth populations, and his destruction of Ukrainian cultural leaders.

As suggested in the preceding paragraph, Conquest is weakest in statistically assessing the demographic trauma of collectivization. Recently, the subject has been debated at length in the *Slavic Review* (43 [1984]: 83–88; 44 [1985]: 505–36; 45 [1986]: 295–313). A key issue, which nondemographers often confuse, is the distinction between "demographic loss" and "excess mortality." On page 300 Conquest argues that the 1937 Soviet census total, when compared to expected growth from the 1926 base, is "decisive . . . in the matter of the total of unnatural deaths." On the next page he modifies this statement by adducing a figure for deficit births, which he seems to consider were limited to kulaks and starving peasants. However, a general, unanticipated fall in the Soviet birthrate, such as has continued during relative prosperity, may already have been affecting the demographic structure. In other words, any calculation of "unnatural deaths" (especially among children) as a residual category is heavily influenced by assumptions about birthrates. Efforts to apply such residual calculations to ethnic Ukrainians separately are even more questionable, because of the unknown extent of migration, reidentification, and changes in census definitions. Such reservations may appear to be quibbles in the face of the undoubted immensity of human suffering, but, as Conquest himself wisely writes (in *Slavic Review* 45 [1986]: 297–98), "The difference is, nevertheless, historically and socially important" because "quantity, as we are told, can change into quality." One may hope, therefore, that future historians, who will derive so much from Conquest's meticulous account, will turn to demographers' discussions of quantification.

JOHN A. ARMSTRONG
St. Augustine, Florida

ROBERT CONQUEST. *Inside Stalin's Secret Police: NKVD Politics, 1936–1939*. Stanford: Hoover Institution. 1985. Pp. ix, 222.

A prolific and versatile scholar practicing outside the traditional precincts of academia, Robert Conquest specializes in the more sordid aspects of Soviet politics, the Stalin era in particular. His *The Great Terror* (1968) has become something of a classic, to its admirers the definitive account of the mass political purge of the 1930s. The present book is essentially a spin-off of that impressive achievement but is dissimilar in scope and content. A slender volume (scarcely over one hundred pages exclusive of appendixes, reference notes, and bibliography), it is more akin to an extended working paper than to conventional history. Thus, despite crisp and often engaging prose, its appeal to the proverbial general reader will be limited. Specialists (all too few in this area) also will not find it easy going, notwithstanding the elaborate checklist of NKVD officers and other useful aids. Nevertheless, the immense labor and expertise involved in extracting the raw material of this elusive subject commands admiration. The sources present problems of reliability, even of authenticity, and purists might argue that the author has been insufficiently critical of the memoir literature, especially that of émigré origin. But, with relatively minor exceptions, this is the first attempt to penetrate the inner workings of the secret police from the fall of its chief, Genrikh Yagoda, in 1936, through the climacteric of Stalin's repression under the sinister Nikolai Yezhov, to the "normalization" of the service under Lavrenti Beria in 1939.

At the risk of appearing ungrateful for this generous bounty of hard and soft data, I perceive some

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gaps and neglected opportunities. The author need not revise his *The Great Terror*—although such a prospect would be welcome—to lend his informed judgment to a number of murky and contentious issues that are still matters of popular and perhaps scholarly ignorance. What of the victims, numerically speaking, of the Stalinist police state? Some younger Sovietologists, impatient with the "totalitarian model" bequeathed by their elders, have begun to question the accuracy of those who would consign untold "millions" to forced labor camps and the execution chambers of the NKVD. On a less momentous note, how does the enigmatic Alexander Poskrebyshev, who headed Stalin's private secretariat (and is sometimes dubbed, with a touch of derisive fantasy, the "Soviet Rasputin") figure in the high-level machinations of Stalin and his police cronies? He does not appear in the text, and the only work seeking to probe the subject (Niels Rosenfeldt's *Knowledge and Power* [1978]) is missing from the bibliography. Stalin himself, inscrutable and unfathomable, remains off stage for the most part, and we are told that his "moves often appear contrary to commonsense" (p. 81). Was he, then, mentally unbalanced, as Nikita Khrushchev strongly hinted? No opinion is expressed, nor are Stalin's motives and objectives examined. But a biography, even a limited one, is not be expected, and we should be appreciative of this pioneering work that may in time lead to a more ambitious undertaking.

ROBERT D. WARTH
University of Kentucky

NEAR EAST

YEHOSHUA PORATH. *In Search of Arab Unity, 1930–1945*. London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1986. Pp. 376. Cloth $32.50, paper $14.95.

The aim of this book, according to the author, is to analyze the interaction between three factors: the Arab attempts to promote their unity, the various endeavors to find a solution for the Palestine problem within the framework of Arab unity, and British policy regarding the first two factors. The title of the book could well have been "Arab Unity and the Palestine Problem." Yehoshua Porath mainly depends on British and Jewish archival material. Although he was able to draw on the Arab press and the memoirs of leading Arab politicians, he acknowledges that his conclusions cannot be regarded as final. The book is perhaps the most detailed discussion of the subject. Its extensive notes would have been more helpful if the author had provided a key to the abbreviations of his archival material, because not all readers know what these initials represent: TC, DBFP, ISA, CS, CP, PREM, CAB, MSC, MEWC, and so on.

The first of the author's five chapters deals with the Hashemite attempts to achieve unity for the Fertile Crescent. It discusses King Faysal's initiatives, Amir Abdallah's Greater Syria project, his contacts with Jewish leaders, and the lack of encouragement from the British. The author also describes the attempts of Nuri al-Said of Iraq to unify the countries of the Fertile Crescent as well as the negative response of the Jews to his offer of concessions and the suspicion and hostility of King Abdul-Aziz of Saudi Arabia.

The second and longest chapter discusses what the author calls "the ever present panacea" or the formation of an Arab federation as a solution to the Palestine problem. The author mentions various proposals made by Arab, Jewish, and British leaders for some kind of Arab federation that would include either a Jewish autonomous unit, province, or token state or a Jewish Palestine. Other British leaders saw no help for the Palestine problem in the scheme of federation. Winston Churchill presented a proposal in May 1941 inspired by the St. John Philby plan for a Jewish state included in an Arab caliphate under the Saudi king, but it was rejected in spite of the efforts of Chaim Weizmann, who incurred the displeasure of several persons by misrepresenting what they had told him. Foreign Secretary Anthony Eden was opposed to it, and President Franklin D. Roosevelt suggested an international trusteeship over Palestine.

The third chapter discusses the rise of pan-Arabism in Egypt and the Fertile Crescent as well as the Arab reaction to the Palestine problem. Chapter 4 explains the British policy regarding pan-Arabism. It repeats in more detail what was mentioned in the last sections of the second chapter and enumerates the proposals made by a long list of British officials.

The fifth chapter discusses the formation of the Arab League from Eden's encouraging statement of February 1943 to the Alexandria Protocol of October 7, 1944, and the final signature of the Pact of the League on March 25, 1945. The author describes the compromises in the pact between various Arab tendencies as well as the annex to the pact regarding Palestine. He also analyzes the attitude of Britain and reveals its influence in moderating the decisions of the Arab participants. Contrary to what many Arabs and Jews believe about British responsibility in the formation of the league, he mentions in his conclusions that Britain did not deliberately encourage its formation but contributed to that process. As for the basic problem in the Palestine dispute, he believes that the autonomy promised to the Jews was much less than what the Zionists were ready to

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accept. The Zionists wanted neither mere autonomy nor a token state but a Jewish state in Palestine, and the developments during and after World War II enabled them to have it.

The author evidently had some difficulties with the vocalization of some Arab names such as Muzhir Raslan (p. 12), which should be Mazhar; Hamid al-Basil (p. 15) should be Hamad; Khalusi al-Khayri (p. 69) should be Khulusi. Some sentences are vague because of faulty construction as in this example, "The Preparatory Committee was not known by this failure" (p. 15). The proofs of the book should have been read more carefully and corrected to avoid such misprints as: "Shakbandar" (p. 28) instead of Shahbandar; British "President" in Amman (p. 8) should be Resident; "Sharif " Arslan (p. 19) instead of Shakib; "Naid" (p. 74) instead of Najd; "swallow" new elections (p. 195) instead of allow.

In spite of these minor and infrequent errors and the lack of better organization of the material, the book is a useful contribution to the history of both the search for Arab unity and the efforts to solve the problem of Palestine during the years 1930–45.

GEORGE M. HADDAD
University of California, Santa Barbara

WM. ROGER LOUIS and ROBERT W. STOOKEY, editors. *The End of the Palestine Mandate*. (Modern Middle East Series, number 12.) Austin: University of Texas Press. 1986. Pp. xi, 181. $20.00.

This slim volume (the outgrowth of a panel at the AHA annual meeting in 1983) contains seven essays by noted specialists on the vexed diplomacy of the final stages of the British mandate in Palestine between the end of World War II and the establishment of the state of Israel in 1948.

Wm. Roger Louis analyzes the Arab and American preoccupations of British policy makers and reveals the world of illusions in which British officials operated. "The game at the United Nations," Louis reports, "was one that the British believed they could win" (p. 13). "The Chiefs of Staff believed that Jewish terrorism could be quelled . . . Attlee believed that firmness against terrorism would strengthen 'the moderates'" (p. 14). "The British . . . hoped that the United Nations would support an independent binational state" (p. 20). Seldom can the policy of a great power have been founded on such a string of misconceptions!

The remaining contributors discuss some of the consequences for other major participants of the British military, political, and intellectual loss of grip in Palestine. Peter Grose tackles American diplomacy; Oles Smolansky deals with the Soviet role; Michael J. Cohen examines "the Zionist perspective" (although his essay also devotes much attention to political maneuvers in the United States); and Walid Khalidi discusses "the Arab perspective." Khalidi's essay suffers from the familiar tendency of writers on this subject to lump together the views of Palestinian Arabs, or even of all Arabs, without differentiation. Thus, we read that "the Palestinians were . . . shocked" (p. 105) by the Royal Commission partition proposals of 1937—although, as Cohen points out on page 95, the elements associated with the Nashashibi clan had in fact expressed serious interest in the partition plan. Happily, in the later part of the paper, more sophisticated distinctions are drawn between the different (and often contradictory) approaches of Palestinian and other Arabs to the conflict. Khalidi gives a particularly interesting account, based on original documentation, of the internecine differences that emerged at the Arab League meeting at Bludan in June 1946. The volume is rounded off by a perceptive "historical overview" by J. C. Hurewitz and a historiographical essay by Robert W. Stookey.

Overall, the book leaves a disappointingly jejune impression. It will not satisfy the general reader since these essays, though sometimes suggestive, often fail to cohere intelligibly. Specialists will probably prefer to turn to the more substantial works in which most of these authors cover the same ground.

BERNARD WASSERSTEIN
Brandeis University

HAMIED ANSARI. *Egypt, the Stalled Society*. (SUNY Series in Near Eastern Studies.) Albany: State University of New York Press. 1986. Pp. xiv, 308.

Hamied Ansari offers a revisionist perspective on the regimes of Gamal Abd al-Nasser and Anwar al-Sadat. He has two main points, and he assembles much data to support them. In the end, however, his claims overstep the evidence.

The first point is that contemporary studies of Egypt exaggerate the autonomy of leader and ruling elite. Few would question that recent studies on contemporary Egypt attach great weight to the personalities and visions of the country's leadership, especially Nasser and Sadat, as the titles of many of these works indicate. Ansari's reminder that leaders do not govern in a vacuum but draw sustenance from a second stratum of political supporters is a useful corrective. His frequent complaints against the work of others have, however, a carping tone and are often grossly unfair. Many of the scholars who stress the critical roles of Egypt's presidents nonetheless give ample weight to the other segments of Egyptian society.

Still, a question remains. Has not the Egyptian ruling elite enjoyed a relatively huge degree of

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independence? Comparing the Egyptian polity with other Third World societies, one must conclude that Egypt's rulers since 1952 have been free from severe internal challenges to their authority and have pursued their own policy goals.

A second, more challenging argument is that Egypt's landed notables, so powerful until 1952, were able to maintain influence in spite of three progressively more severe land reform laws. This possibility has always intrigued observers of the Egyptian scene. Sadat's revival of the private sector and his desequestration measures suggest that the powers of this old landed aristocracy were not shorn. By focusing on records compiled by the short-lived but energetic Higher Committee for the Liquidation of Feudalism, which investigated the continuing political influence and often illegal landholdings of the old landlords, Ansari produces irrefutable evidence that many magnates kept large tracts of land in their own hands (either illegally or through legal ruses) and exercised wide political influence in the countryside and at the national level. Chapters 3 and 4 on the rural elites under Nasser are major contributions to recent Egyptian history.

The author stretches the argument too far, however. It is one thing to demonstrate that many large landowning families from prerevolutionary days remained powerful in the 1960s and 1970s. It is quite another to assert that "no significant transformation occurred in the social and agrarian structures under the Nasserist regime" (p. 134). To make but one obvious comment, the largest landowning family in Egypt by far was the royal family. The redistribution of their massive estates and the surcease of their political influence made a real difference in Egyptian society. It is also not clear from the data assembled by Ansari that some of the other large landowning families (Badrawi-Ashur and Amr, to mention two of the most prominent) retained substantial tracts of land and exercised anything like the influence that they had possessed before land reform.

ROBERT L. TIGNOR
Princeton University

AFRICA

BOGUMIL JEWSIEWICKI. *Marx, Afrique et Occident: Les pratiques africanistes de l'histoire marxiste*. (Centre for Developing-Area Studies, number 19.) Montreal: McGill University Centre for Developing-Area Studies. 1985. Pp. xii, 179. $10.00.

Bogumil Jewsiewicki was one of the editors of the *Canadian Journal of African Studies* during the late 1970s and early 1980s when that journal emerged as a significant contributor to Africanist scholarship. Jewsiewicki thus oversaw and helped shape at least some of the intellectual currents he discusses in this book. But the book is not easy to characterize. It is part historiography and part personal reflection, which accounts for both its insights and its peculiar omissions.

Jewsiewicki begins his essay by asking whether a specifically African Marxism exists. The question is important, particularly because of the ways "Marxism" has been appropriated by some unpleasant political regimes, although nothing is distinctively African about that. Jewsiewicki identifies three faces of Marxism: it is a methodology, a political practice, and a way of viewing the world. Because these three aspects rarely combine, Jewsiewicki is concerned and confused by what has gone under the rubric of Marxism in African studies.

Although Jewsiewicki is sympathetic to Marxism (having added significantly to the debates on modes of production in Africa), he is skeptical of the contribution Marxism has made to African studies and to African political practice. The essay lightly skims over the colonial era. The bulk of his essay deals with the 1960s and early 1970s, when African studies developed its own legitimacy and its own intellectual "anticolonial" struggle. Jewsiewicki is critical of this literature and of the intellectual impact Marxism has had on Africans. He argues that Marxism entered Africa with "modernization" of the 1960s and that it left with "underdevelopment" of the 1970s. In a brief conclusion, which he promises to amplify in a subsequent essay, Jewsiewicki identifies class formation and the transition to capitalism as the two most enduring analytical concepts Marxism has contributed to African studies.

He is best when discussing the Francophone literature, particularly the anthropologists who pioneered the debate on the relevance of Marxism to the study of African societies. He is less competent on the political contribution Marxism has made, except for his personal knowledge of Zaire. He neglects to mention that many leading African nationalists were influenced by Marxism and by the Comintern, because only they offered anti-imperialist theoretical and political programs. That these nationalists abandoned Marxism is an important part of recent African history. Jewsiewicki neglects the role of Marxism in Portuguese Africa, where it shaped both the liberation struggles and subsequent political ones. Jewsiewicki has also underestimated the contribution of Nigerians and Senegalese to the Marxist intellectual and political debates in their countries.

Jewsiewicki's essay is part of a wide-ranging process of reflection and self-criticism in the social sciences and humanities. It should be read in those

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terms. And the footnotes, nearly twice as long as the text itself, provide one of the best compilations of the Anglophone and Francophone Marxist literature available. A six-page English abstract is included.

RICHARD ROBERTS
Stanford University

CLAIRE ROBERTSON and IRIS BERGER, editors. *Women and Class in Africa*. New York: Africana Publishing of Holmes and Meier. 1986. Pp. ix, 310. $55.00.

Studies of women and class in Africa grow out of two strains in African studies: the recent vogue in Marxist analysis, inspired by French scholars who concentrated on precolonial modes of production and who, with some notable exceptions, largely ignored women, and theoretical perspectives from women's studies mainly developed by feminist anthropologists. Until now feminist Marxist work on women in Africa has posited precolonial theoretical models not clearly supported by field research or has concentrated on twentieth-century capitalist relations of production while assuming an undifferentiated precolonial state of "patriarchy." The present book edited by Claire Robertson and Iris Berger takes a giant leap forward by building on these divergent strains to develop new theoretical insights securely linked to empirical research.

A collection of thirteen articles plus an introduction, the book unevenly covers the continent with two surveys, five articles based on research in East Africa, three in West Africa, two in Central Africa, and one in South Africa. All concentrate on class formation in the twentieth century, although most begin by delineating precolonial relations of production. Virtually all make three assumptions about the experience of African women: that the integration of African societies into the world capitalist economy weakened women's economic position vis-a-vis men, that women may not share the socioeconomic status of men who are closely associated with them, and that, in the analysis of class formation, gender relations are highly significant at the household level. Like any collection, the book is uneven in quality. At worst, some authors confound gender with class or nationalism with class consciousness. More often, however, the authors carefully build their theoretical models on data derived from fieldwork and archival research illuminated by relevant Marxist work in European, Latin American, and African studies.

The articles are organized into three sections dealing with access to critical resources, dependence versus autonomy, and female solidarity or class action. Each section begins with a theoretical chapter (by Patricia Stamp, Janet M. Bujra, and Kathleen Staudt) meant to explore the stated theme. Stamp's brilliant blending of theory with fieldwork on the Kikuyu of Kenya uses the concepts of underdeveloped capitalism and sex-gender system to distinguish the African setting from Europe and to set up a neutral theoretical tool to analyze gender relations over time. She argues that women in the main provide the peasant subsidy necessary to underdeveloped capital and offers the unusual hypothesis that women's self-help groups in Kikuyu society represent efforts by women to retain control over their work and its products. Bujra argues that the proletarianization of male labor in Africa leads to an intensification of women's activity in the production and trading of petty commodities. She cites the value of the female subsidy in agricultural and commodity production to explain why, unlike in Latin America and Asia, women have not been drawn into wage labor in Africa. Bujra notes that petit bourgeois women redefine the roles of women at lower levels to serve their own interests, an observation further documented by Christine Obbo and Janet MacGaffey. Staudt's chapter worries over women's inability to build solidarity across class lines and draws a conclusion, unsupported by the articles, that three conditions challenge gender stratification: policies of socialist regimes, the dissemination of gender ideology through the United Nations, and the growth of female participation in the labor force.

Three other chapters are worthy of note. Bonnie Kettel cogently argues that the patriarchal relations of property commonly associated with pastoral societies are the projection of researchers' capitalist world views. She traces the patriarchy apparent today among the Tugen of Kenya to colonially inspired commoditization of productive resources. Finally, MacGaffey's case studies of female entrepreneurs in Kisangani, Zaire, and Jane L. Parpart's fascinating reconstruction of social life on the Zambian copper belt nicely confirm Bujra's hypotheses.

EDNA G. BAY
Emory University

LISA ANDERSON. *The State and Social Transformation in Tunisia and Libya, 1830–1980*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1986. Pp. xxiv, 325. $35.00.

This book includes a thorough study of the political situation in Tunisia and Libya before their control by France and Italy. Numerous studies deal with the politics of Tunisia and Libya after the coming of European powers, but Lisa Anderson's book is one of the few good sources for the earlier political developments of Tunisia and Libya. The author traces how Europeans developed patron-client rela-

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tionships to replace the old tribalism based on kinship in the hinterlands of each country.

When the French invaded Tunisia, they found that the bey's bureaucracy had at least penetrated the coastal cities. The French were able to impose a protectorate, which gave them the political machinery to monitor the bey's old bureaucracy. On this basis the French slowly extended political power to the hinterland. In contrast to Tunisia, in Libya the Italians invaded in 1911 but were not able to consolidate power there until after World War I. The Libyans in the countryside tenaciously held to tribalism and kinship.

Both Tunisia and Libya were nominally under Turkish suzerainty. The beys appealed to Turkey for help against European encroachment, but the North African mamluks, as loyal servants of the Turkish sultan, were able only to act as advisers to the beys. Whereas France considered the Turkish shield easy to penetrate, the powerful Sanusiyyah religious order in Libya allied with the Ottoman Turks against Italy. In both conquered countries the Europeans worked to create a peasantry from the Bedouin society in the hinterland. Establishing an agricultural economy was necessary to allow a peasant-farmer class to emerge. In essence the Europeans attempted to merge the *Bled al Makhzan* (government land) with the *Bled al Siba* ("land of insolence") in the more remote parts of Tunisia and Libya.

Whereas the establishment of a protectorate in Tunisia helped create a stronger economy with a viable trading area, Libya did not offer this stability. Therefore, the European colonists found Tunisia a more hospitable place to emigrate to than Libya.

In Tunisia three developments encouraged the establishment of a strong infrastructure: the activities of the Neo-Destour party, which was able to reach into the interior by creating party cells in the small towns; the creation of a labor syndicate, the Confédération générale des travailleurs tunisiens; and the growth of a Tunisian middle class. These elements formed the basis for French-Tunisian negotiations leading to Tunisian independence under the presidency of Habib Bourguiba in 1956. None of these key developments took place in Libya. However, the discovery of large oil deposits in Libya in 1960 set in motion a movement that finally overthrew King Idriss and brought Mu ammar al Qadhdhafi to power in 1969.

With its extensive bibliography this book helps the North African scholar follow the slow change in Tunisia and Libya that reveals "the social structural and political consequences of state formation" (p. 11).

DWIGHT L. LING
Marietta College

ALLAN CHRISTELOW. *Muslim Law Courts and the French Colonial State in Algeria.* Princeton: Princeton University Press. 1985. Pp. xxi, 311. $40.00.

Rarely does a book plumb its chosen topic so thoroughly and exhaustively as this one does. Allan Christelow chronicles the development of the peculiar form of the Islamic courts of law in French colonial Algeria. For the most part he successfully keeps the regional, personal, and ideological threads clearly distinguishable in what could easily have become an impenetrable mass of data.

Christelow's thesis is that France's demands for economic productivity and political stability forced it to create a new kind of law court in Muslim Algeria. Unlike some other colonial societies in Africa, Algeria had a tradition of written law and of legal experts. Somehow this had to be contended with by the colonial power. In Algeria the French decided to place the qadi courts in a new and strict hierarchical and bureaucratic structure, which maintained its coherence despite the colons' protests that Islam was only being concretized within the state. The new courts gave up the process of consultation with the ulema for a new, formal appeal structure. By this reform the French created a native hierarchy beholden for its status to the colonial regime. At the same time, the Islamic court became a mainstay of nativist legitimization of the Islamic religious and historical tradition against the colon, even though the court was an integral part of the colonial regime.

By recognizing and legitimizing Islam in the court structure, the French deftly parried a potential religious source of resistance by marabout-led Muslims in the west of Algeria. But, when the religious faction was neutralized, the ulema, through the majlis, saw their own political authority being contested. When their resistance failed, they turned to the new system themselves and soon became the backbone of it. In eastern Algeria the Ottoman empire had established control over both the religious and the ulema, and the French more easily slipped into the former empire's shoes.

To neutralize the religious influence of the marabouts and to break the consultative control of the majlis, the French raised the status of the qadi, and he came to be the authoritative representative of religious devotion and legal authority, a role the qadi had not enjoyed for centuries, if at all. The French backed their system by creating a number of medersas, schools designed to train the qadi in Islamic religion and law, as well as in Western thought. In the 1870s and 1880s resistance by the colons and by their political allies in France to Islamophile policies was put down, and the Islamic court system integrated more fully into colonial rule.

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Christelow explores these themes in every detail and in every direction. The author uses history, geography, sociology, and political science to describe and explain the dramatic changes that French colonial rule made in the Islamic legal culture. The mass of data the author has assembled will daunt readers as they travel, chapter by chapter, through this work of history and sociology. Some of it is, frankly, tough slogging, but most of it carries the reader, stage by stage, through this complex story. This is a book for the specialist and will remain a classic exposition of the Islamic legal structure in Algeria for decades.

DAVID F. FORTE
Cleveland-Marshall College of Law

KRISTIN MANN. *Marrying Well: Marriage, Status, and Social Change among the Educated Elite in Colonial Lagos.* (African Studies Series, number 47.) New York: Cambridge University Press. 1985. Pp. xii, 194. $39.50.

By the late nineteenth century a small and fairly cohesive group of Christian, Western-educated Africans formed the dominant elite in colonial Lagos, the capital of modern-day Nigeria. Members of this elite had to choose between two different forms of marriage: Yoruba marriage, which was practiced by the majority of the population, and Christian marriage, which was strongly advocated by British officials and missionaries and increasingly came to define elite status. A number of elite men practiced both forms simultaneously, marrying one woman in a Christian ceremony and having other wives married according to Yoruba tradition. The choice of marriage patterns had different implications for elite status, domestic relationships, and inheritance, and the marriage dilemma deeply concerned elite men and women, who expressed their feelings both privately and publicly.

Kristin Mann explores the history of marriage among the African elite in Lagos over a thirty-five-year period spanning the turn of the last century. Mann focuses her research on the lives of two hundred elite men, defined by their occupational roles, and their wives, sisters, and daughters. She has made extensive use of oral data to flesh out the life histories of each of these men. In addition to newspapers and archival and missionary records, she productively uses such documents as wills, marriage registers, and court records, which contain a wealth of relevant data.

In rural Yoruba areas, having many wives and children was a valuable economic and political asset. In colonial Lagos, however, the importance of extensive kin networks declined as legitimate commerce became the mainstay of the economy and the private ownership of land and capital emerged as important economic resources. Mann argues that Christian marriage, with its emphasis on monogamy, concentrated valuable resources within the elite and created a dense network of affinal relationships.

Both men and women found the Victorian ideal of the intimate, companionate marriage difficult to achieve. Elite women came to feel particularly constrained by expectations that they stay home, be economically dependent on their husbands, and focus their energies on being proficient homemakers and moral exemplars. Elite men sometimes resented the inheritance provisions of "ordinance" marriage, which gave Christian wives and children distinct (and nontraditional) property rights in cases of intestacy. Ultimately, both husbands and wives rejected Victorian expectations about marriage and evolved new patterns of marital relationships that drew on both Christian and Yoruba tradition.

Mann's innovative attempt to tackle African social history and extensive oral and documentary research have resulted in a work that is strikingly original on at least two counts. First, whereas most writing in African history is, of necessity, blandly anonymous, this book is full of distinctive men and women whose names reappear throughout the text. (Indeed, this slim volume is apt to receive extensive use in Nigeria as a *Who's Who* of Lagos in 1900.) Second, Mann is able to show how dramatic social and economic changes appeared to the people who experienced them, and her arguments are built on the words of the participants themselves.

Succinct, well-written, and clearly argued, Mann's book is a pioneering contribution to the history of African marriage and family life.

MARGARET JEAN HAY
Boston University

MARTIN CHANOCK. *Law, Custom, and Social Order: The Colonial Experience in Malawi and Zambia.* (African Studies, number 45.) New York: Cambridge University Press. 1985. Pp. xi, 286. $42.50.

In an excellent, tightly argued book Martin Chanock brings fresh insights to the analysis of law, anthropology, and history in colonial Malawi and Zambia. Enlarging on and refining previous work, Chanock exposes the constructed nature of African customary law and effectively destroys the myth that this legal code embodies unsullied traditional practices from the African past. Chanock documents the evolution of customary law from a fluid process of social reconciliation to a rigidly codified set of laws designed to protect the interests of rural chiefs. In a devastating critique of British functionalist anthropology, Chanock rejects the assertion of fundamen-

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tal similarities between African and Western legal processes and argues instead that, rather than reflecting traditional practices, African customary law was created by African leaders to define and protect only those values that served their interests.

The evolution of customary marital law in Zambia is a particularly interesting case in point. Rural African leaders insisted that Africans had always severely punished those who transgressed marital custom; adulterers were killed, for example. Yet Chanock discovered that adulterers in the precolonial period more often escaped with a fine. This preference for the more draconian version of the African past is understandable once one discovers the function of the new customary laws. Rural African elders used the myth of a stable past to assist them in their quest to control increasingly independent young men and women. Powerless to stop male migration, these leaders sought at least to keep women at home. Complaints of rural deterioration and female intransigence received a sympathetic hearing from colonial officials bent on ruling through rural chiefs. As a result, colonial officials were only too willing to help African rural leaders redefine and strengthen customary laws designed to limit female options by discouraging divorce, restricting female migration, and fining adulterers.

Chanock also raises some important questions for feminist historians of Africa. The colonial period has been seen as one of decline for women, but Chanock's data suggest a more nuanced interpretation. He describes an initial period when women's options increased as colonial officials tried to free African women from "repugnant" traditions such as child marriage and bride price. This had important consequences as many women used their new jural and economic opportunities to escape rural partriarchal authority. Although this freedom diminished with the resurgence of African rural authorities in the 1930s, women continued to fight for control over their lives. Although Chanock underestimates this struggle, he has opened a fruitful new area for research. This alone makes this sometimes dense and difficult book worth reading. But its insights into "customary law" should make it required reading for policy makers as well as historians.

JANE L. PARPART
Dalhousie University

THOMAS O'TOOLE. *The Central African Republic: The Continent's Hidden Heart.* (Profiles. Nations of Contemporary Africa.) Boulder, Colo.: Westview. 1986. Pp. xiv, 174. $28.50.

Thomas O'Toole presents a succinct survey of one of the least-known countries in Africa. The volume opens with two broad background chapters on the country's geography and precolonial history, but its strength resides in a contemporary history of the troubled years since independence. Topics include recent political history (chap. 3), social issues (chap. 4), and political economy (chap. 5). Two final chapters catalogue the country's foreign relations and assess scenarios for the future.

Victimized by incompetent leadership since independence, the Central African Republic (CAR) is most often identified with the bizarre and macabre antics of Jean-Bedel Bokassa, who ruled the country successively as *général, président à vie*, and emperor from 1966 to 1979. O'Toole offers several intriguing hypotheses that place these troubled years—as well as the stints of misrule before and since—in a broader historical context. First is the suggestion that the dictatorial regimes of the independence era are but extensions of the arbitrary rule of the colonial period: the "new" nations of Africa are the products of Western autocracy, not democracy. Second, he explores the neocolonial role of France and how French interests have benefited from and encouraged irresponsible rule in this former colony. O'Toole also interprets recent history of the CAR in terms of the theoretical perspectives of the Zairean scholar Ilanga-Kabongo and suggests that the governing clique may best be seen as a "bureaucratic bourgeoisie" that uses ethnicity and the power of the state to extract the surplus needed to maintain itself. This in a country where production, the only source of true wealth, remains under foreign control. The author pushes this interpretation to its extreme, suggesting that Bokassa's actions represented a "privatization" of the state itself. Finally, the survey includes several disturbing hypotheses about contemporary society: that the people are perhaps now in poorer health than in the precolonial era and that migration and urbanization, the rise of the informal sector, and the loosening of familial ties have undermined socialization in the cities and thus further contributed to social chaos.

Although this book is the best on the CAR available in English and although the hypotheses presented above could be profitably used to interpret the recent history of other former colonies, the study has shortcomings. For example, to say that daily routines in the *kodros*, urban *bidonvilles*, are "fundamentally those of a rural village" (p. 87) or that life in the rural areas continues unaltered (pp. 91, 93) overemphasizes continuity and runs counter to the main argument of the book. And to suggest that the CAR, a country nearly the size of France populated by about three million people, is a potential "demographic timebomb" (p. 82) ignores the demographic history of the region: the population before the late nineteenth century was probably greater than it is today, with no ill effects. The

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"demographic shock" that brought population decline thereafter mostly resulted from the political economies of slave raiding and colonialism, just as the threat to the health of the people of the CAR today mostly results from the political economy of neocolonialism. Overpopulation is not a problem. Finally, the promise of chapter 6, "The CAR and the World," would have been better fulfilled by an analysis of the place of the country in the international political economy than by a simple listing of dealings with foreign powers. More comprehensive analysis is essential to understand the historical evolution of the CAR and to rescue it from "continued personal rule," which O'Toole concludes is the most likely scenario for the future.

DENNIS D. CORDELL
Southern Methodist University

ALBERTO SBACCHI. *Ethiopia under Mussolini: Fascism and the Colonial Experience*. London: Zed Books; distributed by Biblio Distribution Center, Totowa, N.J. 1985. Pp. xxvi, 262. Cloth $29.50, paper $10.75.

Fifty years after Italy's invasion of Ethiopia, we still lack a full treatment of the event. Departing from the usual emphases on political negotiations and military campaigns, this work targets Italy's colonial policy in Ethiopia. The scope is not comprehensive; Alberto Sbacchi addresses only the nature of Italy's imperial administration, the role of Italian settlement, and personal Ethiopian-Italian interaction.

Fitting Italian colonialism into a broader African context, Sbacchi finds comparison, only to discover that Italy's Ethiopian adventure was little more than a historical footnote, marking a last chapter in Europe's scramble for Africa. It came when pressures to decolonize the continent were already building. World War II, of which the Italian defeat in the Horn was a part, was thus also the beginning of colonialism's end.

Sbacchi studies a colonial policy that barely existed. Italian objectives in Ethiopia were never clearly defined or directed. Mussolini, hoping for a diplomatically negotiated protectorate, was consequently ill-prepared to conquer or administer Ethiopia. His colonial officials, largely self-serving, often confronted and undermined each other. Italy's inadequate resources were embarrassingly evident. The Italian government failed to recruit enough administrators to effectuate direct rule; neither was Italy able to guarantee adequate salaries or rewards to co-opt significant numbers of Ethiopian collaborators. Plans for Italian settlement in Ethiopia proved unworkable; few families arrived, and they were denied essential administrative and economic support. Clearly, Italy's colonial policy in Ethiopia, like its military control, was largely wishful thinking.

On the whole, Sbacchi is sympathetic to Ethiopia's plight and seeks to avoid a "Eurocentric" focus. Yet he heavily relies on Italian archival sources and personal accounts, so much so that one wonders how much such one-sided reliance has jeopardized his independence of mind. Sbacchi transcribes Italian conclusions from the documents almost as if they were his own and fails to demarcate carefully his assessments from theirs. Occasionally, he falls prey to colonial racist concepts; he refers, for example, to the "native" population (p. 56) and describes the Oromo as "Hamitic" (p. xi). Elsewhere, Sbacchi notes a "backwardness" among the peoples of southwest Ethiopia, a deficiency apparently associated with certain stateless societies there and the region's supposed unattractiveness for Italian settlement (p. 133). Whose assessment is this? It certainly reflects a basic ignorance of the region.

This book complements our advancing knowledge of European colonialism in the Horn but must be read with caution. Much remains to be uncovered in our assessment of the social and economic features of the Italo-Ethiopian War. But, ultimately, Sbacchi is most disappointing in his failure fully to address some basic questions of Ethiopian historiography: was Italian colonialism's failure related to internal Ethiopian dynamics associated with residual feudal or evolving modernistic structures, and was persistent Ethiopian resistance to Italian domination reflective of "embryonic nationalism"?

CHARLES W. MCCLELLAN
Radford University

CHRISTOPHER P. YOUÉ. *Robert Thorne Coryndon: Proconsular Imperialism in Southern and Eastern Africa, 1897–1925*. Waterloo, Ontario: Wilfrid Laurier University Press; distributed by Humanities, Atlantic Highlands, N.J. 1986. Pp. xiv, 241. $35.00.

Robert Coryndon was the longest-serving governor in British colonial Africa. For twenty-eight years (1897–1925) he served as the top administrator of North-Western Rhodesia (now western Zambia), Swaziland, Basutoland (Lesotho), Uganda, and Kenya. Christopher P. Youé insists that such a career cannot be dismissed, as some anticolonial historians would have it, yet he hardly portrays Coryndon as a hero, as some in the imperial school would suggest. His emphasis is almost entirely on policy; he writes little about Coryndon's personal life.

Coryndon earned a reputation among Europeans as a firm, no-nonsense administrator; for many Africans, of course, this merely meant that he well embodied the brutality of colonial rule. In

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Swaziland he was known to Africans by the nickname *Msindazwe*, or "oppressor" (p. 60). Historians have generally viewed him as a champion of European settlers. This is reasonable, since he presided over grossly unequal divisions of land between blacks and whites in Swaziland and Southern Rhodesia, encouraged European planters in Uganda, and quashed the bid for political rights by Asians in Kenya. Youé argues, however, that Coryndon was not "dogmatically" prosettler and adduces considerable evidence of his support for African peasant production. Coryndon's abiding interest (besides law and order), Youé shows, was in harnessing the efforts of Africans for the benefit of the imperial capitalist economy. Both wage labor on European enterprises and peasant cash-crop production had their place.

The notion of Africans pursuing "separate development" did not have a place. Despite Coryndon's role in land division, Youé argues subtly that he was not a true segregationist. He did not endorse "separate identity" or "parallel development" either sincerely (like many missionaries) or cynically (like South Africa's rulers). For Coryndon land separation was a temporary expedient. He favored gradual assimilation of Africans into the bottom rungs of a single society, and he believed this would improve them. Although not a segregationist, then, he was surely a racist, and he believed in European political and cultural supremacy.

Given the importance of land policy, that Youé makes no mention of Coryndon's actions in this sphere during his first assignment, in North-Western Rhodesia, is curious. Here, Coryndon's enthusiasm, within limits, for white settlers was first evidenced. Despite this omission, the book, largely based on records of the Colonial Office, is a valuable contribution to colonial history. It is thoroughly researched, clearly written, and generally sensible in its judgments.

KENNETH P. VICKERY
North Carolina State University

KENNETH INGHAM. *Jan Christian Smuts: The Conscience of a South African*. London: Weidenfeld and Nicolson. 1986. Pp. xii, 284. £14.95.

Kenneth Ingham has provided what may well be the definitive study of Jan C. Smuts, twice prime minister of South Africa, an enormously influential statesman during the interwar period, and an ardent segregationist. This highly readable and meticulously researched biography gives us a refreshingly new and more balanced perspective of an extremely complex and little-understood political figure.

Ingham takes us on a fascinating journey through the life of Smuts, from youth to old age. In separate chapters we see him as a student, young lawyer, young soldier, minister and general, international statesman, prime minister, opposition leader, and collaborator. Through an exhaustive examination of Smuts's private correspondence, Ingham reveals a man with a brilliant legal mind, a remarkable capacity for work, and an ability to synthesize ideas and information. Although proud of his Afrikaner roots, Smuts was an ardent South African nationalist and a fervent believer in the British empire. Ingham unveils the inner thoughts that animated these seemingly lofty convictions. Throughout his life Smuts dogmatically clung to the idea that only through white unity could white domination be maintained. And only through white supremacy in South Africa and in the world beyond could Western civilization be preserved and promoted. This fixed notion prevented Smuts from coming to grips with the realities of race in South Africa. He used his remarkable mind and energy to uphold racial segregation, deny nonwhites political power, and promote white immigration and Asian expatriation. Ingham reveals that Smuts was a confirmed Zionist and asserts that he persuaded the British to establish a Jewish homeland in Palestine. Smuts supported the cause not out of a concern for anti-Semitism but because he regarded Jews as a civilizing factor in Western society for centuries. Moreover, a Jewish nation in the eastern Mediterranean could protect imperial interests at Suez and beyond.

With brilliant insight Ingham unveils the enormous contradictions in Smuts's thinking. Here, he suggests, is a man "who relied so confidently upon the powers of the human intellect when dealing with international problems . . . but used different criteria when considering the relations between Africans and Europeans in South Africa" (p. 189). In an intensely paternalistic and arrogant manner, Smuts refused to consult with even the most educated Africans on issues that affected them. Fearing that black power implied the ultimate destruction of Western civilization, he opposed the franchise for nonwhites. In his dealings with whites as well as with blacks, Smuts tended to be impatient, obdurate, and rigid.

In private life this rather solitary individual was a family man of simple taste who loved the company of women and delighted in reading philosophy and the Bible. But Ingham has also unearthed an individual fascinated by flowers and plants, not for their beauty but for their botanic properties. And here, too, emerges a statesman of powerful intellect and broad international vision who in Europe helped formulate the League of Nations yet at home in South Africa worked to divide the races and to prevent the realization of freedom and equality for all. In every sense Smuts comes through this lucid narrative as one of modern history's most tragic

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figures. Ingham suggests that at crucial junctures Smuts had the power and overseas connections to move his country in a different direction. Instead, he used these assets to perpetuate injustice and in doing so set his beloved nation on a collision course.

This is a compelling book and should be essential reading for anyone seeking to probe deeply into the complex mind of whites in South Africa today.

RICHARD W. HULL
New York University

SHULA MARKS. *The Ambiguities of Dependence in South Africa: Class, Nationalism, and the State in Twentieth-Century Natal.* (Johns Hopkins Studies in Atlantic History and Culture.) Baltimore: Johns Hopkins University Press. 1986. Pp. ix, 171. $17.50.

Shula Marks has skillfully combined three lectures, originally given to the Atlantic History Seminar at Johns Hopkins University in 1982, into an integrated historical study. This short and elegantly written study is of far-reaching interest because of the theoretical base on which the author develops historical interpretation and analysis. Her approach springs from a dissatisfaction with the existing literature on South Africa. She sees recent work on the political economy as marked by heavy structuralism that leaves little room for the role of the individual, whereas studies of particular nationalist and trade-union organizations are heavily institutional and only deal with individuals at a generalized and ideological level. The book vividly portrays the changing nature of domination in South Africa through its emphasis on the ambiguity of meaning and structure. Each essay is shaped around an episode in the life of an individual and demonstrates levels of ambiguity.

The individuals considered are Solomon ka Dinuzulu, the son and heir to the last Zulu monarch, whose position was recognized by his people but not officially by the South African state; John Langalibalele Dube, third-generation Christian, epitome of the "new African," and first president in 1912 of the South African Native National Congress (which later became the African National Congress); and Allison Wesels George Champion, a politician and a leader of the Industrial and Commercial Workers' Union (ICU), the largest black trade union in South Africa until recent times. Solomon was not deposed by the white authorities despite insulting behavior before the earl of Athlone, the governor-general of South Africa, and general drunkenness and dissolution that would have resulted in the removal of any other chief. Dube moved from being the leading spokesperson for a pan–South African nationalism to representing a Zulu ethnic nationalism. Champion was banished from Natal, not because of instigating beer hall riots in 1929, nor because of his leadership of the ICU, but because of police reports that incorrectly asserted that he was forging an alliance with Solomon.

In a short conclusion Marks examines the important contemporary South African Zulu leader, Mangosuthu Gatsha Buthelezi, in whom she sees, united in one person, the ambiguities of all three of her subjects. "Like Solomon, Dube, and Champion, Buthelezi is a master of the politics of the tightrope" (p. 6).

Marks has more than accomplished her goal of demonstrating that history offers the best critical vantage point to view the present. Her essays "unashamedly" center on events and individuals and capture action and structure through them. This book will be of value to historians, political scientists, anthropologists, and all who are concerned with the complexities of South Africa.

PATRICK O'MEARA
Indiana University

ASIA AND THE EAST

JOSEPH NEEDHAM. *Science and Civilisation in China.* Volume 6, *Biology and Biological Technology*; part 1, *Botany.* Assisted by LU GWEI-DJEN and HUANG HSING-TSUNG. New York: Cambridge University Press. 1986. Pp. xxxii, 718.

We are three decades into Joseph Needham's massive project, *Science and Civilisation in China*, and this is the twelfth volume to appear. It resembles its precursors in being an exemplar of the booksmith's craft, copiously illustrated and footnoted and containing exhaustive bibliographies in Chinese, Japanese, and Western languages. As always, it is a collaborative effort involving many scholars but bearing Needham's unmistakable stamp.

Even Needham's seven-hundred-page tome is too small to compass the history of plant sciences in China. Consequently, between these covers we find only one-half of the project's section on botany. The second half, assigned to one of Needham's collaborators, is in progress. Agriculture, nutrition, and pharmaceutics are assigned to still other sections. Yet even with the scope thus narrowed, this is a survey rather than an exhaustive treatment of the subject.

The book is subdivided into four major subsections: plant geography, plant terminology and nomenclature, the history and typology of Chinese botanical literature, and pesticides and other biological aids (this subsection written by Huang Hsing-Tsung). Subsections reserved for the sequel volume include: classification; description; plant physiology, sexuality, parasitism, pathology, and so on; as well as

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horticultural techniques and Chinese influence on modern botany.

Needham has produced an "epic" of Chinese botany. According to him, ancient China produced no scholar of plants to equal Aristotle's successor, Theophrastus. The Chinese, however, drew on their much richer flora to pioneer in several fields. Their early literature dealing with plant geography and soils was far more developed than that of other civilizations. Also, a technical, albeit unsystematic, vocabulary emerged to deal with plant morphology and taxonomy. The normal learned way to refer to plans consisted of binomes: doublets that typically comprised a generic word coupled with an adjective denoting the subcategory, a form parallel to the genus-species terminology ultimately stabilized by Linnaeus in the eighteenth century.

Whereas the Greek achievements were followed in the West by a long medieval dry spell, China experienced no such prolonged break in its tradition. Needham describes, in his second subsection, the steady florescence of botanical literature into the eighteenth century. Of particular interest are the pharmacopoeia or *ben cao* (using here standard pinyin rather than Needham's romanization), which culminated in the *Ben-cao Gang-mu* (The Great Pharmacopoeia) of 1596, that "pendectal treatise on mineralogy, metallurgy, mycology, botany, zoology, physiology, and other sciences." Also of interest are categories of botanical literature unrivaled elsewhere: namely, the monographs on specific plant groups (for example, bamboos and peonies) and, second, famine manuals about emergency food plants.

China's botanical high point came in the sixteenth and the seventeenth centuries. But before this autochthonous development could go further, before East Asia might produce its own Linnaeus, if indeed that were likely, ecumenical science spread from its European cradle and overtook all of the world's learned traditions. Even so, China's achievements were remarkable, including practical knowledge such as biological pest control.

Among the merits of this volume are the author's relish for the subject and his exuberant erudition. He scatters fascinating material at every turn. Examples include his observations on the role of Latin and Greek in Western science and his case for replacing classical botanical nomenclature with Chinese characters; the story of his acquisition of what may be the oldest printed book in England; an account of the eighth-century peony-mania comparable to Holland's seventeenth-century tulip-mania; the evocation of the brilliant, doomed court of Emperor Hui Zong of the Northern Song; and his speculation about the connections between the fourteenth-century prince and esculentist Zhu Xiao and the Jews of Kaifeng.

Needham's runaway erudition has, however, a negative side. It gives the work—already freighted with botanical terminology, Chinese philology, and digressive footnotes—a meandering quality. For example, little is gained by dragging the reader through the inconclusive etymology of early Chinese botanical terms. And even Needham's interesting case for replacing classical nomenclature with Chinese characters seems quixotic, more appropriate for a long footnote or a separately published editorial.

Also to be wondered about is Needham's theoretical orientation to his subject. Historians of Western science have, for twenty years, made use of the "paradigm" concept first developed by Thomas Kuhn. Needham seems uninterested in that idea and preserves a more traditional incrementalism. This disinclines him to discuss the conceptual schemes of Chinese naturalists, how they differed and changed, and the forces that might have promoted or inhibited a shift from one to another. This is so, even though he discusses or at least mentions systems of classification, scientific language, the Confucian concept of *ge wu* (investigation of things), and other Chinese theories. Since the second half of the botanical section will contain a subsection on classification, Chinese botanical paradigms may be more thoroughly examined there.

In the meantime this work booms forth as another grand salvo against the kind of Eurocentric ignorance that allowed S. Wells Williams to say in 1841 that "Botany, in the scientific sense of the word, is wholly unknown to the Chinese" (p. 1).

CRAIG DIETRICH
University of Southern Maine

DAVID FAURE. *The Structure of Chinese Rural Society: Lineage and Village in the Eastern New Territories, Hong Kong.* (East Asian Historical Monographs.) New York: Oxford University Press. 1986. Pp. xviii, 273. $32.00.

Maurice Freedman referred to a wealth of secondary studies to advance several propositions about the origin, characteristics and development of the Chinese lineage. He also used the concept of lineage to theorize about how corporate lineages and state institutions interacted to minimize or increase social instability in rural society. Focusing chiefly on Kwangtung and Fukien, Freedman claimed that lineages accumulated property and their members worshiped a designated ancestor. Lineages varied in wealth and power, and the more powerful lineages were likely to evolve in regions of a high potential economic surplus. Freedman's writings on lineage spawned a new generation of fieldwork and doc-

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toral theses about Chinese lineages in the Hong Kong and Taiwan regions.

David Faure's study expands our understanding of Chinese lineages in the New Territories by his extensive field research and use of primary records from villages. Although difficult to read, Faure's work breaks new ground.

He makes the important point that a village lineage can only be understood by a thorough history of that village's settlement: namely, how village residents acquired their original property rights, expanded those property rights, and legitimated their right to worship an ancestor. Lineage evolution depended on whether the members held "bottom-soil" or "top-soil" property rights. Lineages holding bottom-soil property rights seemed more likely to acquire wealth, develop alliances with other lineages, and expand their territorial influence. Although sensitive to the different types of early property rights held by members of a lineage, Faure does not develop a typology to explain different patterns of lineage development according to different types of property rights.

Faure also argues that village rituals for worship cut across lineage lines. Community worship usually involved commemorating the village temple and celebrating various temple gods. Villages related their temple gods to some heavenly hierarchy of officials and defined the physical territory of village worship in terms of how villagers were to be protected. Lineage and village societies had their separate dimensions of human behavior in Chinese rural society.

Finally, Faure contends that the patterns of settlement and evolution of villages in the Hong Kong region, as well as their complex lineage forms, probably were very similar to those in other regions of China, especially the southeast.

This lively, insightful work will stimulate much debate among scholars of Chinese anthropology and sociology.

RAMON H. MYERS
Hoover Institution on War, Revolution and Peace

JOHN W. CHAFFEE. *The Thorny Gates of Learning in Sung China: A Social History of Examinations.* (Cambridge Studies in Chinese History, Literature, and Institutions.) New York: Cambridge University Press. 1985. Pp. xxii, 279. $39.50.

John W. Chaffee's *The Thorny Gates of Learning in Sung China* is one of the most important books to appear on premodern Chinese history in the last decade. China's examinations and literati elite have been studied by many scholars before him, yet Chaffee offers much that is fresh and challenging. Taking for granted that most students came from landholding families, Chaffee concentrates on the next, crucial nexus: how wealth was translated into prestige and power in a society that reserved so many of its rewards for those who studied the classics, took examinations, and became officials.

As Chaffee documents, the Chinese educated elite grew rapidly during the Sung period (960–1279). Early in the eleventh century about twenty to thirty thousand students took the lowest level (prefectural) examination; by the end of the century seventy-nine thousand did; in the mid-thirteenth century, when the Sung only held two-thirds of the former empire, an estimated four hundred thousand did. Even though about half of all officials continued to be recruited through "protection" (nomination by a relative in office), the prestige of examination degrees had become firmly established.

Chaffee places the increase in size of the elite in a dynamic context, showing the consequences of successive developments over the three centuries of Sung rule. Governmental policies—especially the advancement open to degree holders and the establishment of hundreds of government-run county and prefectural schools—fostered the increase in candidates. In the more economically and educationally developed regions of the country, competition at the prefectural level was intense, and failed candidates numerous, aiding the development of literati and examination culture in those areas. At the same time, since there were no regional quotas for the highest degrees (as there would be in Ming-Ch'ing times), the less well developed areas had trouble competing at the national level and were not proportionately as well represented in the bureaucracy. As competition intensified, men did everything they could to improve their odds; they moved to the capital, falsified their residency, entered the National University, and so on. Moreover, many modifications were made in examination procedures, especially in the provision of separate, special, or facilitated examinations, which helped the relatives of officials gain degrees. By the thirteenth century, in areas with high concentrations of privileged people, a large majority of those with the highest degree (*chin-shih*) had not taken the regular prefectural examinations but special facilitated ones.

In his treatment of these subjects Chaffee shows himself to be a mature scholar. He offers cogent summaries of complex institutional developments, devises imaginative ways to analyze the statistical evidence available, draws careful inferences from anecdotes, and moves comfortably between contemporary Chinese interpretations and his own analyses. At all times he keeps in mind both larger national trends and regional variation and writes

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with grace and clarity. I have seldom read a book in which I found so much to admire.

PATRICIA EBREY
University of Illinois

MATTEO RICCI. *The True Meaning of the Lord of Heaven (T'ien-chu Shih-i)*. Translated by DOUGLAS LANCASHIRE and PETER HU KUO-CHEN. Edited by EDWARD J. MALATESTA. (Jesuit Primary Sources, first series, number 6.) St. Louis: Institute of Jesuit Sources or the Ricci Institute for Chinese Studies, Taipei. 1985. Pp. xiv, 485. Cloth $39.00, paper $34.00.

M. HOWARD RIENSTRA, editor. *Jesuit Letters from China, 1583–1584*. Minneapolis: University of Minnesota Press. 1986. Pp. 47.

These two translations present very different sides of the Jesuit experience in China during the years 1579–1607. One is a modest, but well-done, work of less than fifty pages that has been published in a limited edition of 500 copies. The other is a collaborative effort of substantial length and difficulty whose appearance marks a milestone in the study of Sino-Western relations.

The letters that have been carefully translated and annotated by M. Howard Rienstra reveal the efforts of the missionary fathers Michele Ruggieri, Francesco Pasio, and Matteo Ricci to gain a first foothold on the Chinese mainland. These letters in Italian were initially addressed to Jesuit superiors in Rome and were later abridged and censored by editors in Rome as part of the standard procedure of preparing them for inclusion in the annual Letterbook of 1586. Whereas the originals expressed the struggles of the missionaries to accommodate the Gospels to Chinese culture, the published versions altered the text to speak of refuting (*confutano* rather than *accomodato*) the principal sects of China (p. 41). Rienstra claims that the published versions sought to convey to Europe a picture of a triumphant and culturally superior Christianity. By contrast Ricci, building on the work of Ruggieri, wrote *T'ien-chu Shih-i* (The True Meaning of the Lord of Heaven) for an audience of Chinese intellectuals as part of an attempt to introduce Christian concepts to China. Here the attempt to accommodate European Christianity to Confucianism (though not to other Chinese teachings) is clearly evident.

Among the most significant works in modern (post-1500) Sino-Western relations, Ricci's *T'ien-chu Shih-i* represents one of the first literary encounters between the cultures of China and Europe. More specifically, it was an encounter between, on one side, the Chinese teachings of Confucianism, Buddhism, and Taoism and, on the other side, Christianity as expressed in Scholastic-Aristotelian reasoning of one of the finest minds produced by sixteenth-century European learning. Ricci blended Confucian terminology with Scholastic meaning and introduced a number of key Scholastic ideas to China, such as the Aristotelian Four Causes. The work was composed in a literary form common to that time, namely, a dialogue between a "Chinese scholar," essentially a Confucian literatus, and a "Western scholar," the mouthpiece of the Christian missionary Ricci. Were this work a catechism, as has so often been misleadingly claimed, it would not bear the historical significance that it does. In fact, *T'ien-chu Shih-i* is a filtered composite of the various kinds of dialogues that Ricci was then engaging in with Chinese literati. Although the work shows how Ricci was introducing Judeo-Christian ideas to the Chinese, it also reveals Chinese concepts Ricci failed to understand, particularly concepts that dealt with Neo-Confucianism and Buddhism.

Douglas Lancashire, Peter Hu Kuo-Chen, and Edward J. Malatesta have produced a bilingual edition with the Chinese text of the first edition (1603) on the left side and the corresponding English translation on the right. They have supplemented this by referring to the third edition (1607). This is not an easy work to render intelligible for today's reader. The work is filled with archaic Chinese translations of names and terminology associated with Scholasticism. The translators have excellently rendered the difficult Chinese text into English, and their expertise is repeatedly demonstrated by their ability to contrast Ricci's Chinese terminology with modern Chinese equivalents. The introductory material, which describes the complex evolution of the work and its various editions, is very enlightening.

The major weaknesses of this work stem from the compilers' misconception that *T'ien-chu Shih-i* can be presented as a semipopular work with contemporary appeal rather than as a scholarly translation that emphasizes the work's historical significance. Because of this view, footnotes and commentary have been minimized. This view also may explain why the prefaces by Feng Ying-ching (1601) and Li Chih-tsao (1607) as well as the epilogue of Wang Ju-shun (1607) have not been translated. Unfortunately, the type of Scholastic reasoning found in *T'ien-chu Shih-i* is so alien to today's reader that extensive commentary is necessary to make the text come alive. Because of this omission, the work is less intelligible and more tedious to read than it need be. Other scholarly aids are also missed, such as an index and the use of Chinese characters to identify certain less familiar terms and names cited in the introductory materials. The reason for listing the titles of Chinese works in English translation without the Chinese eludes me.

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Nevertheless, the attitude of the compilers is highly objective toward Ricci. This is far from being hagiography, and Ricci's flaws of comprehension and European bias in regard to the Chinese are carefully noted. Rather than seeing Ricci's work as any final statement, they appear to see it as a historical effort that provides a stepping stone for further dialogue between the contemporary "Chinese scholar" and "Western scholar." Their work shows that the spirit that animated Ricci is still very much alive.

D. E. MUNGELLO
Coe College

LYNN A. STRUVE. *The Southern Ming, 1644–1662*. New Haven: Yale University Press. 1984. Pp. xvii, 297. $30.00.

This is the first general account in Western literature of the political history of the Southern Ming. Whereas other scholarly studies have focused on the Ch'ing side (notably Frederic Wakeman, Jr.'s, *The Great Enterprise* [1985]), Lynn A. Struve has analyzed Ming resistance to Manchu conquest from the side of the conquered. The disintegration of the Southern Ming is attributed to the incessant conflict between civil (*wen*) and military (*wu*) officials, a problem the Southern Ming had inherited from the early Ming government. With admirable literary skill, Struve presents the complex melodrama in five stages, depicting how the Southern Ming had lost from one "stand" to another until the death of the last "emperor"—as an effective institution the "emperor" long since "had died and been sloughed away" (p. 195).

Political, fiscal, and economic chaos complicated by frequent popular riots characterized the "first stand" of the Hung-kuang regime. The ten-day massacre at Yang-chou committed by the Manchu conquerors and the corresponding heroic resistance of the Ming loyalists highlighted the "first defeat" of the Southern Ming. During the "second stand" chronic lack of coordination between civil and military leaderships continued to weaken the position of the Southern Ming courts. While the Ch'ing pushed on in its conquest of the southeast and far south, the Southern Ming regime became increasingly dependent on the military, resulting in the "second defeat." Here, Struve, citing both Chinese and Japanese sources, presents an interesting account of the unsuccessful attempt of Cheng Ch'eng-kung to solicit military assistance from Tokugawa Japan. In the chapter entitled "Resistance and Third Stand" Struve describes the dramatic upturn in the Yung-li court and subsequent collapse of the Southern Ming forces in Chiang-hsi and Hu-kuang, setting the stage for Manchu conquest of the far south. "Third Defeat and Standoff" deals with two separate themes outside the Southern Ming court: the death of Chang Hsien-chung, the one major rebel leader since Li Tzu-ch'eng, and the collapse of his "Great Western Kingdom"; and the story of Cheng Ch'eng-kung's secret negotiation for a surrender to the Ch'ing and their ultimate failure to reach an agreement. The chapter "Last Stands and Last Defeats" marks the conclusion of the study as well as of the Southern Ming regime. In the southwest the regime ended with the tragic execution of the last Ming dynast, Yung-li, in May 1662. In the same year Regent Lu died on the island Chin-men off the Fu-chien coast. In the extreme southeast Cheng Ch'eng-kung, after his "northward campaign" had failed, retreated to Taiwan with his remaining forces and, having expelled the Dutch from the island, established a "Ming Eastern Capital." By then, despite the symbolic significance of the Cheng regime on Taiwan, Southern Ming had formally come to an end.

The book contains vivid portraits of leading figures in the story. It also provides seventeen maps that help the reader follow complex political and military developments. Modifications in details would have been inevitable had Struve had the opportunity of full access to rare Ming texts and archival sources in China. As it now stands, however, Struve's work signifies a major contribution to the field of Ming-Ch'ing transition. It will be an indispensable reference source for future studies of the Southern Ming.

SILAS WU
Boston College

YEN CHING-HWANG. *Coolies and Mandarins: China's Protection of Overseas Chinese during the Late Ch'ing Period (1851–1911)*. Singapore: Singapore University Press. 1985. Pp. xvi, 413. $17.00.

In this work Yen Ching-hwang argues that, after an initial period of ignoring both emigration and emigrants, the government of the Ch'ing dynasty followed a policy of protecting overseas Chinese. He asserts that statements that the Ch'ing had no policy toward overseas Chinese (which he finds to be pervasive in existing literature) originated in anti-Ch'ing propaganda. Yen admits that the policies had little substantial effect in ameliorating the treatment of the overseas Chinese. But he locates the reasons for the failure in the overall weakness of China rather than in inattention or the incompetence of Ch'ing diplomatic approaches.

Yen narrates a complex story, covering sixty years and the Chinese emigration to Singapore, Malaysia, Peru, Cuba, the United States, and South Africa. Conditions among the Chinese communities in

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these countries varied greatly, from the relatively rich and powerful businessmen in Singapore and Malaysia to plantation workers (in Cuba and Peru) and miners (in South Africa) who lived under conditions little better than slavery. Indeed, the impetus for the coolie trade was the need for cheap labor after the abolition of the African slave trade. Before 1860 Chinese subjects were forbidden to emigrate, so that the elaborate coolie trade that developed in the years following 1845 was illegal and underground. As a result of treaties with the Western powers signed in 1860, emigration became legal, although the Ch'ing law prohibiting it remained on the books until 1893. Ch'ing recognition of the significance of the overseas communities was made manifest in a law of 1909, stating that all males of Chinese ancestry, no matter where their birthplace, were citizens of China.

Yen demonstrates ways in which the Ch'ing government used the techniques of modern diplomacy to secure its goals. Consuls were sent abroad to protect Chinese subjects and to foster their identity as Chinese. Treaties that sought to protect overseas Chinese and assure their right to return to China were negotiated with the Western powers. When reports reached the Ch'ing government that Chinese in Cuba and Peru were ill-treated, commissions were sent from China to investigate. After the Rock Springs Massacre in Wyoming in 1885, when twenty-eight Chinese miners were killed, the Chinese government protested, demanding (and receiving) compensation from the U.S. government.

Yen is most successful in his discussion of individual diplomatic initiatives. The options and frustrations confronting individual diplomats such as Ch'en Lan-pin and Wu T'ing-fang, who were faced with issues as diverse as anti-Chinese bigotry in California and working conditions on Peruvian plantations, are vividly portrayed and demonstrate both the potential and the limitations of late Ch'ing foreign policy. Yen argues that foreign policy and emigration policy are linked, but he does not persuasively demonstrate how they were linked in the minds of the chief actors. The domestic political situation serves as a constant background to Yen's story, but, it is a background that should occasionally be brought into sharper focus. The radical politics of at least some of the overseas communities is surely relevant to Ch'ing policy toward them yet receives scant mention here, perhaps because Yen has written on the subject in his *The Overseas Chinese and the 1911 Revolution* (1976). There is much material in the book that will be of interest to diplomatic historians and to persons interested in overseas Chinese communities.

ANN WALTNER
University of Minnesota

JOEY BONNER. *Wang Kuo-wei: An Intellectual Biography.* (Harvard East Asian Series, number 101.) Cambridge: Harvard University Press. 1986. Pp. xv, 314. $30.00.

Wang Kuo-wei (1877–1927) is remembered for his brilliant studies in philosophy and ancient Chinese history, for a seemingly perverse Ch'ing loyalism, and for his suicide. The present volume discloses the inner coherence of these various expressions of Wang's personality.

That personality defined itself against the background of late Ch'ing and early republican nationalism, with its progressive abandonment of traditional values in the pursuit of wealth and power. Disdaining such superficial utilitarianism, Wang pursued the more profound questions of what is true, good, and beautiful. Western armies and parliaments impressed him little, but he was greatly taken by the philosophies of Kant and Schopenhauer. Then, after exposure to Nietzsche killed his enthusiasm for metaphysics, he turned to art as humanity's most ennobling activity. During the last years of the dynasty he worked for the Ministry of Education but primarily devoted his energies to poetry and the critical appreciation of Yüan drama.

The integrity expressed hitherto in Wang's commitment to learning and art was expressed in a commitment to traditional cultural values after 1911. He abhorred the brutal slayings of Ch'ing officials and opportunistic betrayals of the royal family during the revolution. He followed Lo Chen-yü, his patron of many years, into exile in Japan and joined in Lo's effort to preserve China's heritage through scholarly investigations of its past. Regretting his subversive excursions into Western philosophy, Wang now used Lo's immense private library to conduct his brilliant researches into ancient texts and oracle bone inscriptions. Returning to Peking, Wang served as an appointed companion to the deposed emperor and shared the imperial household's humiliations at the hands of Feng Yü-hsiang's minions in 1924. When Feng threatened to seize Peking again in 1927, Wang went one morning to the Summer Palace's Kunming Lake, "the only clean spot left today" (p. 212), and drowned himself, a martyr to the principle of loyalty in an age of opportunism.

Joey Bonner's sympathetic portrait of Wang is all the more valuable for its acute critical observations on his understanding of German philosphy, translations of and commentary on his poetry, and review of his ingenious theories regarding Shang genealogy. But intellectual historians might wish for a wider contextualization, for others reacted similarly or differently to the degeneracy of the age. Comparison with the suicides of Liang Chi, Ch'en

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T'ien-hua, and T'an Ssu-t'ung would have been instructive; conservatives had no monopoly on high-minded martyrdom. Nor, as Liang Shu-ming and the early Lu Hsün show, was superficial modernization the object of attack from only one quarter. Why did some preserve their integrity by a pure devotion to philosophy or Confucian principle, whereas others did so by revolutionary self-sacrifice? Why did some blame shallow opportunism on the erosion of traditional values, whereas others saw the tradition as a corruption of human nature? Readers concerned with Wang's historical significance and the broader determinants of his intellectual and spiritual odyssey are left to make such comparisons for themselves. Certainly, this book makes an important contribution to our efforts to answer these questions.

DON C. PRICE
University of California, Davis

ALVYN J. AUSTIN. *Saving China: Canadian Missionaries in the Middle Kingdom, 1888–1959.* Toronto: University of Toronto Press. 1986. Pp. xvii, 395. $27.50.

Diverse in personalities and complicated in motives, the story of Protestant and Roman Catholic Canadian missionaries to China is aptly related by Alvyn J. Austin in this book. From the pioneer missionary on Taiwan, George Leslie Makay, who pulled forty thousand teeth during his career, to the last missionaries arriving home in the 1950s, who were instructed by their mission boards at the point of disembarkation to write and say nothing about China, this is a story of naiveté, mission bureaucracies, foreign policy, and colorful characters.

On the Protestant side, Mackay, a Zorra boy (Canadian Horatio Alger), had his work publicized by the mission board, which Austin claims acknowledged Mackay's "madness" (p. 32). Mackay drove all Western women, and consequently their husbands, from his field yet took the unusual step, in the 1870s, of marrying a Chinese convert. Inspired by Mackay, Jonathan Goforth arrived in China in 1888 to distribute famine relief funds, only to learn in Shanghai that lacking any knowledge of Chinese he would be no help. After language study Goforth settled into the Honan mission, whose tumultuous presbytery meetings lasted longer than those of the General Assembly of the entire Canadian Presbyterian Church (p. 45). Goforth's Chinese contacts included Feng Yu-hsiang, the Christian general, who immediately became the hero of mission literature at home. Later, when missionaries warned that Feng had changed, the mission board decided not to publicize the fact to supporters—leaving many Canadians with a mission-colored picture of political events in China.

Roman Catholic missionaries from Quebec offered a far different approach to mission work. Among the Catholics the evangelist-missionary was rare. They depended instead on establishing a center where young missionaries completed their training before ordination or final vows and to which Chinese might come to be instructed in Christianity and, perhaps, converted. Shaped by their experience in British-dominated Canada, the missionaries from Quebec viewed China differently than did other Westerners. On seeing Chinese in an anti-British demonstration, one Jesuit remarked that it looked like Quebec (p. 152), and another during World War II denounced the Kuomintang as "freemasonry" and praised the Japanese for fighting communism (p. 257).

Like their American counterparts who attempted to understand Chinese politics during World War II, some Canadians spoke openly and truthfully of the situation and, consequently, ran afoul of their government's policies toward China.

Austin covers the development of hospitals, the relationship of the missionaries to the Chinese revolution of 1911 and its aftermath, and their roles in Japanese-occupied China. He also covers such footnotes to China mission history as the short career of Aimee Semple (later McPherson) in Hong Kong and the role of the missionaries in building Chinese museum collections in the West.

More descriptive than analytical, Austin's book is written in a popular, narrative style with numerous anecdotes. Based on Protestant and Catholic archival sources, it presents an often neglected contrast between these two groups of missionaries. The Canadian missionaries' role in China has been little studied, making this work a welcome addition to mission literature.

KATHLEEN L. LODWICK
Southwest Missouri State University

GAIL HERSHATTER. *The Workers of Tianjin, 1900–1949.* Stanford: Stanford University Press. 1986. Pp. viii, 313. $37.50.

This is a useful, sometimes moving analysis, in the tradition of E. P. Thompson, of the lives and cultures of the men and women who came from the countryside to labor in the mills and streets of a major industrial city of northern China.

Tianjin was quite different from China's best-known industrial center. "In Shanghai cotton was king and cotton mill workers were his handmaidens . . . The predominance of women in the Shanghai textile industry has led to an image in both popular and scholarly works of the woman cotton mill

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worker as the quintessential victim and heroine of pre-Liberation industry." But in Tianjin women "entered the cotton mills later and in (much) smaller numbers than their southern counterparts" (p. 55).

As a result, rather than producing a parallel to Emily Honig's rich account of female workers in the Shanghai cotton mills, Gail Hershatter has written a well-balanced treatment of a less famous industrializing city with distinctive characteristics and problems. Three case studies deal with the lives and cultures of workers in different sectors: ironworkers with their harsh apprenticeship system; cotton mill workers caught in a system demanding control of their lives and working days; and freight coolies entangled in a fearful maze of gangs and violence.

Women were a part of the textile industry in Tianjin, and Hershatter's account is rich in detail and passionately written. As a minority within the workforce, women faced sexual exploitation as well as the harsh discipline common to developing textile industries around the world. Resistance was based on networks established according to gender or place of origin but rarely went beyond work slowdowns, picturesquely known as "soaking mushrooms." Outcomes were inconclusive. Until 1949 mill owners never got a stable workforce, and mill workers, male and female, were never fully transformed into an industrial proletariat.

A good discussion of working-class markets and amusements, of crime and prostitution, and of ordinary celebrations of life's passages rounds out the book. Little stability was to be found in the city, whether for warlord or worker, but a final chapter on working-class protest is yet another footnote on the wisdom of Mao Tse-tung's strategy of rural base. Through case study Hershatter has vividly demonstrated that Tianjin's working "class-in-itself" could not develop into a "class-for-itself" during this period.

Although the book's two maps are barely adequate, Hershatter has mined her largely Chinese-language source material skillfully and written a valuable addition to the growing literature on urban China in the twentieth century.

ANGUS W. MCDONALD, JR.
Minneapolis, Minnesota

YUNG-FA CHEN. *Making Revolution: The Communist Movement in Eastern and Central China, 1937–1945*. Berkeley and Los Angeles: University of California Press. 1986. Pp. xxiv, 690.

Chalmers Johnson's *Peasant Nationalism and Communist Power* (1962) and Mark Seldon's *The Yenan Way in Revolutionary China* (1971) are known for their monistic interpretations of the Chinese Communist party's (CCP) mobilization of peasants during the Sino-Japanese War (1937–45). Yung-Fa Chen's volume is, by contrast, a matter-of-fact study of the complex realities of wartime interaction between the CCP and the rural population behind the Japanese lines in Kiangsi and Anhwei provinces, where the party faced challenges not only from the Japanese but also from the Nationalists, the traditional rural elite, and other contending local forces.

Unexcelled in the use of primary sources and secondary works, including a sizeable collection of relevant CCP internal documents, Chen describes and analyzes, in great detail, the social and political contexts in which the party operated. He similarly examines the specific issues, programs, tactics, and organizational tools that the party used for winning friends and undermining enemies. He accomplishes these tasks in the course of reviewing the sophisticated processes through which the party founded and used such vitally important grass-roots organizations as mass associations, rural governments, militia, and party branches in the region in question.

Chen clearly shows that the CCP's wartime success in the countryside was not the result of any single cause as Johnson (peasants' search for anti-Japanese leadership) and Seldon (the CCP's socioeconomic reform programs) have contended. Rather, it was achieved because of the party's masterly exploitation of a host of pressing as well as latent issues facing various groups of the rural population. Chen also makes clear that, given many peasants' political apathy resulting from ignorance, timidity, habitual acceptance of traditional values and power structure, and the like, the CCP might not have so successfully rallied the rural masses behind itself if it had not brought to the region dedicated cadres proficient in organizational and agitational work and capable of implementing the "united front" policy with "subtlety and finesse."

Chen's eminently realistic study is somewhat marred by one minor internal inconsistency and by several one-sided comments at the end of the book. The internal inconsistency occurs in Chen's treatment of tenancy: whereas in chapter 3 he says that about 60 percent of the rural residents in the region were full- and part-time tenants (p. 134), in the concluding chapter Chen states that the "landless proletariat" there was numerically small and that tenancy was therefore only a minor issue in the mobilization of peasants (pp. 516–17).

Chen's less-than-balanced statements include his entirely negative assessment of the effect of the CCP's wartime mass mobilization on post-1949 Chinese sociopolitical developments and his unsubstantiated indictments against agricultural collectivization and the now much maligned Cultural Revolution. Space does not permit me to examine these peripheral flaws individually. Insofar as the CCP's wartime success in achieving unprecedented

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political penetration is concerned, Chen clearly errors in overlooking the effects of that penetration in enabling the CCP to politically reunify the Chinese mainland, rid it of many deeply rooted sociopolitical ills and injustices (at least until Mao Tse-tung's death), achieve initial industrialization mainly through self-reliance, and restore China to a respectable position in the family of nations after 1949.

FRANKLIN W. HOUN
University of Massachusetts

TAI-CHÜN KUO and RAMON MYERS. *Understanding Communist China: Communist China Studies in the United States and the Republic of China, 1949–1978.* Stanford: Hoover Institution. 1986. Pp. xi, 172. Cloth $17.95, paper $9.95.

This slim volume purports to compare the "explanatory power" of research on the People's Republic of China (PRC) ("Communist China") published in Taiwan with that published in the United States over the past three decades. American studies of the PRC come out the loser by far in the opinion of Tai-Chün Kuo and Ramon Myers.

Although that conclusion is allegedly based on a careful analysis of "major China studies journals, monographs, and conference volumes" (p. 136), alas the authors often appear to have selected examples to support the conclusions they favored. They did not conduct a systematic or replicable analysis of the entire range of scholarly materials available in either country. And the five "descriptive typologies" that they claim to test for their predictive and interpretive powers—to the asserted gross disadvantage of American scholarship—are too simple-minded to be useful.

Unfortunately, in some American scholarly circles research about the Chinese mainland by scholars in Taiwan has been ignored and unfairly devalued as merely parroting a political anti-PRC line. Acknowledging that erroneous view does not, however, give license to its mirror image. In some matters of "court history" or what the Chinese traditionally term *wai-shih*, that is, historical "investigative reporting," scholars in Taiwan clearly have an edge on most American students. And by various means they have been able to procure critical documents from the PRC not otherwise available. But with respect to range and quality of analysis—and also to quantity of output—the overall American scholarly record has no peer.

ALBERT FEUERWERKER
University of Michigan

JAMES HUNTLEY GRAYSON. *Early Buddhism and Christianity in Korea: A Study in the Emplanation of Religion.* (Studies in the History of Religions, number 47.) Leiden: E. J. Brill. 1985. Pp. viii, 164.

In the preface to his study of the Korean adoption of religions introduced from abroad, James Grayson confesses that he intends "neither to elucidate the history of a particular religious tradition nor to make a contribution to Korean studies." No one would accuse him of undue modesty.

Grayson traces the process whereby Buddhism, Catholicism, and Protestantism took root on Korean soil. He organizes his tale around an argument that religions introduced into alien cultures experience stages of contact and explication, penetration (which can be either slow penetration or rapid conversion), and expansion before they become firmly established in their new land. He relates in minute detail how the three religions introduced into Korea first reached the peninsula, how Koreans responded to them, and finally how Buddhism, Catholicism, and Protestantism became Korean religions. He then attempts to squeeze those details into his model of a three-stage process of "emplantation."

Unfortunately, his model adds little to previous recitations of the facts. No explanatory power is gained by labeling the initial phase of the encounter between Buddhism and Koguryŏ culture, or that between Catholicism and Yi dynasty Confucians, a stage of "contact and explication." Grayson should have simply told his story and refrained from cluttering it with tautological terms. He also should have proofread his manuscript more carefully. References to a Mongol invasion of Korea in the seventeenth century (p. 135) and to an Italian Jesuit named Johannes Rodriguez (apparently, he means the Portuguese Jesuit João Rodrigues) (p. 65) do little to enhance a reader's trust in the other names and dates that fill this work.

Overall, this book is a disappointing addition to the meager library of English-language studies of premodern Korea.

DONALD BAKER
University of British Columbia

DAVID POLLACK. *The Fracture of Meaning: Japan's Synthesis of China from the Eighth through the Eighteenth Centuries.* Princeton: Princeton University Press. 1986. Pp. xv, 250. $32.50.

This is a provocative book. David Pollack addresses one of the most fundamental questions in Japanese cultural history: the meaning of "China." As he carefully states at the beginning of his study, he is not so much interested in Chinese cultural influences on Japan per se as he is in how the Japanese perceived Chinese culture and used China in the creation and development of their own culture.

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Pollack sees "China" and "Japan" as antithetical elements in a dialectical process that underlay Japanese cultural evolution for a thousand years. He uses various terms to discuss this process but perhaps most frequently describes it as a relationship or interaction between "native content" and "alien form." The "fracture" of content and form, as Pollack identifies it, occurred at the beginning of literacy in Japan, when the Japanese began to adapt the Chinese script to write their language. In a chapter devoted to the *Kojiki* and the problem of writing and in many other discussions of literature throughout the span covered by the book, Pollack perceptively and convincingly demonstrates that the Japanese have been profoundly ill at ease with literacy and that the burden of writing in a script taken from an alien language has increased their preference both for orality in literature and for unarticulated rather than articulated expression.

Pollack's subject is of such vast scope that he can only deal with it selectively in a book of this length. The topics he examines are *The Tale of Genji*, the *Shinkokinshū*, early medieval Zen, *wakan* (the mixing of Japanese and Chinese) in *renga* (linked verse) and other genres of literature of the fourteenth and fifteenth centuries, and Tokugawa aesthetics. I was particularly impressed with the chapter "The Informing Image: 'China' in *The Tale of Genji*." This is a fascinating discussion of the seemingly odd eclecticism of the Japanese in selecting their models from Chinese culture (often reaching in directions that would have been incomprehensible to the Chinese, if the Chinese had bothered to look) and of the use of China as a striking background for the thoroughly Japanese story of *The Genji*.

I was least enthusiastic about "The Intellectual Contexts of Tokugawa Aesthetics." Although the chapter contains many good ideas, Pollack has, I believe, spread his net too broadly. In commenting on a great variety of painters, writers, and philosophers, he does not provide complete or convincing arguments to support his basic thesis.

Yet, overall, this is a book of fine quality. Pollack displays impressive erudition in his knowledge of both Chinese and Japanese culture and writes with grace and wit. I hope that the book serves to stimulate further research and writing, by Pollack and others, on the China factor in premodern Japanese culture.

PAUL VARLEY
Columbia University

MARIUS B. JANSEN and GILBERT ROZMAN, editors. *Japan in Transition: From Tokugawa to Meiji*. Princeton: Princeton University Press. 1986. Pp. ix, 485. $47.50.

For many decades Japanese historians who study the Meiji Restoration of 1868 have vacillated in characterizing it. Some call it a process of "reform" (*kaikaku*), others a "revolution" (*kakumei*). The favorite label of recent choice is *henkaku*, which signifies a "transformation" but avoids the ideological baggage of "revolution." Now comes a joint volume by sixteen authors who wish to describe "the changeover from Tokugawa (1600–1868) to Meiji (1868–1912) in nineteenth-century Japan" (p. 3). The editors choose to characterize this changeover as a transition because it "was anticipated in Tokugawa and it extended into Meiji" (p. 23). This is a tall order, for the transition in question covers the years 1850–90 and involves a recognized watershed in Japanese and world history. That this book is largely a success is a credit to everyone concerned with it.

The tale of transition is told in four parts. After a survey by the editors comes part 1, consisting of four articles on central and local administration. Next there are four articles on established institutions: Buddhism, the military, education, and the press. Then come four articles on population and cities. Finally, part 4 offers four articles on rural life and material culture. The sixteen authors hail from four continents and a dozen universities. They do not speak with one voice, because they represent no single school of interpretation. Their concerns are multiple, their methods eclectic. The book features a social scientific orientation, and in two of the four parts extensive use is made of quantitative methodology.

Some of these articles survey and review existing knowledge. For example, Albert Craig on the central government and Marius B. Jansen on the ruling samurai class produce synopses of processes whose outlines are well established. Other articles explore areas on which general knowledge is short. Akira Hayami on population shifts, Gilbert Rozman on castle towns, Kozo Yamamura on the Meiji land-tax reform, and Osamu Saitō on rural economy examine problems that have previously received little monographic attention in Western scholarship.

Still other articles go beyond existing studies. Andrew Fraser's study of administrative reform in Awa-Tokushima reveals anew why models of change based on Tokyo do not suffice for other places in Japan. Richard Rubinger's article on changes in the educational system opens an important field of social history. Henry Smith's study of Edo-Tokyo affords a striking view of transition in the capital. And Susan Hanley provides the first explanation of the stability of life styles in Japan.

The articles by Michio Umegaki, Eleanor Westney, Albert Altman, William Wray, and Shunsaku Nishikawa also contribute to the book's overall purpose. All the authors use primary sources as well as secondary studies, and they have pro-

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duced original, well-turned essays. The result is a useful volume on Meiji Restoration history constructed from the standpoint of the social sciences.

This book nevertheless has a problem, which lies not in what is here but in what is missing. The great transformation that was the Meiji Restoration featured not only measurable social and economic aspects but also cultural and intellectual change. In this volume only Martin Collcutt's path-breaking essay on Buddhism under threat of extinction even touches religion and ideology. Nothing else addresses shifts in thought and belief. The experience of common people is also ignored, despite the preponderance of commoners among Japan's population of 35 million in the 1870s. The word *yonaoshi*, signifying millenarian "world renewal" and popular efforts in the 1860s to achieve it, does not figure here. The present volume thus sketches a "transition" apparently smooth in its progress—and bereft of the ideas involved in the revolutionary dimension that also marks the Meiji Restoration.

GEORGE M. WILSON
Indiana University

THOMAS A. STANLEY. *Ōsugi Sakae, Anarchist in Taishō Japan: The Creativity of the Ego.* (Harvard East Asian Monographs, number 102.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press. 1982. Pp. xviii, 232. $20.00.

In 1923 Osugi Sakae, his mistress, and her nephew were strangled by a military police captain. The manner of Osugi's death and his status as one of Japan's few self-styled anarchists have earned him mention in most histories of modern Japan. The work reviewed here is nonetheless the first full-length biography of Osugi in English. Unfortunately, this rather thin work (172 pages of text) does little to establish the historical significance of its subject.

Thanks to an unusually frank autobiography, particularly by Japanese standards, Thomas A. Stanley is able to provide great detail on Osugi's adolescence, including his enthusiasm for masturbation. Unimportant family details, probably not very different from those for thousands of other youths at the time, receive as much or more attention as Osugi's exceptional politics. Indeed, Stanley has little to say about the writing of the autobiograpy itself even though it is one of the most remarkable Japanese works of this genre.

Stanley's narrative is rigidly chronological. A too brief description of Osugi's conversion to anarchism is followed by a discussion of his renunciation of homosexual activity—not because of any organic relation between the two but simply because they belong to the same years of his life. Although the author has apparently read many anarchist and socialist works, at least those explicitly or probably read by Osugi, he does little to locate his subject in some left-right continuum of revolutionary thought, nor does he seriously relate Osugi to other radical thinkers who grappled with questions of individual freedom and ego.

Perhaps the most interesting portion of the book describes Osugi's "relaxed, even friendly" (p. 83) relations with the police who followed him about. Because of the manner of his death, to find that Osugi used his police shadows to run errands is most striking. Comparison of relations between the police and radicals before and after Osugi's time or between the United States and Japan would have made for a more interesting and informative study, but the author essentially stops at providing anecdotes.

This is a consistent defect of Stanley's approach. Comparisons that would illuminate Osugi's thought or his time are simply not made. For example, Osugi's problems in trying to reconcile his petitbourgeois intellectual status with sympathy for workers were hardly his alone, but Stanley stops with the specifics of Osugi's case. Essentially, the reader is presented the data of Osugi's life and left to put these in some context.

Overall, Stanley renders colorless and lifeless the activities of an individual who thoroughly scandalized his contemporaries, recognized and grappled with some of the most significant contradictions that confront radical activists, and achieved recognition as a one-man threat to imperial Japan. Osugi and the period deserve more.

EARL H. KINMOUTH
University of California, Davis

MACALISTER BROWN and JOSEPH J. ZASLOFF. *Apprentice Revolutionaries: The Communist Movement in Laos, 1930–1985.* (Histories of Ruling Communist Parties.) Stanford: Hoover Institution. 1985. Pp. xiv, 463. $13.95.

The paucity of scholarly work on a country as important in international politics as Laos has been since World War II might seem curious. This excellent new study of the Communist movement in Laos by MacAlister Brown and Joseph J. Zasloff suggests that scholarly neglect results at least in part from the scarcity and uneven quality of the sources, as well as from the inaccessibility of the country.

The unevenness of the sources is apparent as one reads the book. The brief first chapter, on the historical background before 1930, leans on the secondary literature, such as it is, and would be

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better skipped by those who know something about the country. Chapter 2, on the Indochinese Communist party and Laos between 1930 and 1940 includes very interesting material from the French *Surété* records, which, however, are closed after 1939. Lao and French memoirs and contemporary Lao accounts are the chief sources for the next three chapters, on the World War II period, the struggle for independence, and the early Vientiane governments to 1959. The final four historical chapters, which extend virtually to the present, are heavily based on personal interviews, official sources, and, from the mid-1960s, the authors' experience. The second half of the book, a thematic treatment of post-1975 government in Laos, is based on sources ranging from Lao governmental statements to the testimony of refugees. Here Brown and Zasloff treat leadership and the party, political institutions, the economy, social policies, and external relations.

Others have written about the history of Laos during this tumultuous half-century, some at much greater length, but none with this depth and wealth of information. The once-shadowy Pathet Lao, or Lao Patriotic Front (*Neo Lao Hak Xat*), is now more clearly visible and understandable, and our view of the country is not as excessively Vientiane-centered as it has been in the past. We now at least have a few more names and faces—and yet these still are cardboard ("red cardboard," perhaps) figures—to place against Souvanna Phouma, Prince Phetsarath, and Oun Sananikone, and, if historians want more, they will at least realize from the bibliographical notes how unhelpful the sources are for putting flesh on these bones. (One major exception is an excellent appendix reproducing correspondence of 1949 between Prince Souphanouvong and the Lao Issara government in Bangkok.)

Two key themes emerge from this study, one explicitly and the other perhaps not. The first has to do with the contribution of the combination of French rule and the Indochina wars to making a reality of an initially artificial Indochina, even while nationalism was pulling in three separate directions—Vietnamese, Cambodian, and Lao. At the same time, their numbers and weight gave the Vietnamese a preponderance that persists even to the present. Second, although the authors do not directly address this question, Laos does not seem to have existed as a nation until a Vietnamese-dominated Communist party forced it into being.

I must add a few minor points. "Annamites," used through the first few chapters, is an inappropriate term for Vietnamese, and the book should have been better proofread and indexed.

This is clearly the best work on the history of modern Laos. Because of the authors' extensive use of interviews with veterans of the Lao Communist party, it is not likely to be superseded.

DAVID K. WYATT
Cornell University

ABDUL KARIM. *Social History of the Muslims in Bengal (Down to A.D. 1538)*. 2d rev. ed. Chittagong: Baitush Sharaf Islamic Research Institute. 1985. Pp. xiv, 305.

This monograph traces the fortunes of Muslims in Bengal from the initial Turkish conquests of the 1190s to the Mogul intrusion of the 1530s. It is not a simple story, perhaps best described as a series of experiments through which an alien elite sought to establish and maintain its control over this remote appanage of the *Dar al-Islam*, remote not only from the heartlands of Islamic civilization but even from the Delhi sultanate, whence it had been grafted. Superficially, developments in early Muslim Bengal seemed to center on dynastic and factional rivalries and on that ubiquitous Indo-Muslim preoccupation with group identity. Below the surface, however, the desire to dominate, and remain distinct from, the subject infidel population was constantly threatened by more subtle processes of cultural assimilation.

In his expanded and updated revision of a monograph that appeared in 1959, Abdul Karim endeavors to eschew a narrowly dynastic or military approach to the history of Muslim rule in Bengal in favor of an attempt to reconstruct the social setting in which Islam first penetrated Bengal and then put down permanent roots, a task for which he is preeminently qualified. Karim has published extensively on the history of Bengal and is probably best known for his biography of the eighteenth-century Mogul governor, Murshid Quli Khan, as well as for important studies in Indo-Muslim numismatics. It is, therefore, not surprising that the strength of the present volume lies in his interpretation of three of the principal categories of sources available to the historian of pre-Mogul Muslim India: numismatics, epigraphy, and Sufi hagiography.

A mine of carefully collated information, this book will prove extraordinarily useful for students of Muslim India to refer to and consult. On these grounds it is to be most strongly recommended. At another level, however, it illustrates all too clearly the isolated and idiosyncratic tradition of Indo-Islamic historiography, which was first established in the nineteenth century by Anglo-Indian writers, and which has outlived the raj. That old adage among historians that getting the right answers depends on asking the right questions remains as true as ever. The tradition of scholarship of which Karim is a distinguished exponent remains

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strangely impervious to much that has happened in the profession during the past forty years.

GAVIN R. G. HAMBLY
University of Texas, Dallas

J. M. S. BALJON. *Religion and Thought of Shāh Walī Allāh Dihlawī, 1703–1762.* (Studies in the History of Religions, number 48.) Leiden: E. J. Brill. 1986. Pp. viii, 221.

Shah Wali Allah (or Waliullah) of Delhi was an important eighteenth-century Islamic reformer. He was the son of a leading mystic of late Mughal India and a prolific scholar who studied in Mecca and Medina during a two-year stay. He then returned to Delhi and devoted his life and writings to reconciling conflicting currents in the Islamic thought of his time.

He was a mystic who was initiated into all the sufi orders then present in India, including the Chishtis and the Naqshbandis, frequently regarded as on the opposite ends of the philosophical spectrum. In one of the mystical debates of the time, concerning the difference between *wahdat ul-wujud* (unity of essence or ontological monism) and *wahdat us-shuhud* (unity of experience or phenomenological monism), Waliullah maintained that a reconciliation of these theories of mystical experience was possible.

Like a number of Naqshbandis before him, Waliullah felt that reliance on mystical experience had led many Muslims too far from basic knowledge of, and reliance on, the revealed message of God as contained in the Koran and *hadith*. Hence, by his scholarship, he attempted to revive the study of the scriptures among Indian Muslims. He also translated the Koran into Persian. He helped bring about a *rapprochement* between mysticism and the Sharia, as well as a reconciliation of the various currents within mysticism.

The present study of Shah Waliullah's religion and thought by J. M. S. Baljon is not a biography of the eighteenth-century divine, nor is it a chronological study of his works. It is, rather, a topical study of his ideas, which deals with revelation, metaphysics, mysticism, ethics, prophetology, and so on. Baljon justifies this arrangement on the basis of the frequently overlapping or repetitive nature and sometimes unclear chronology of Shah Waliullah's works. The work is thus less than ideal from a historian's point of view, for it provides little sense of the temporal or political contexts of these ideas. In addition, the work demands extraordinary assiduity from the reader (as do Shah Waliullah's works). In fairness to Baljon, however, he succeeds well in his declared purpose, which is to present a comprehensive survey of Shah Waliullah's works, allowing him to speak for himself. The works are quoted frequently in translation, with copious annotation. Baljon has appended a glossary of technical terms that is indexed for ease of reference. This compact work is thus a reference guide to Shah Waliullah's ideas, extremely useful to anyone with an interest in the intellectual history of Islamic reform.

GAIL MINAULT
University of Texas

YASMINE GOONERATNE. *Relative Merits: A Personal Memoir of the Bandaranaike Family of Sri Lanka.* New York: St. Martin's. 1986. Pp. viii, 270. $22.50.

Recent years have witnessed an outpouring of volumes by British authors, often nostalgic, concerning the days of the British raj in South Asia. Few have attempted critical evaluation or showed an interest in the indigenous population subject to imperial rule.

The volume under review provides a valuable contrast to the tales of the raj just mentioned. An educated and literate member of an upper-stratum clan in the elite segment of Sri Lankan society, Yasmine Gooneratne portrays the values and viewpoint of the indigenous elite. Her memoir of her distinguished clan is written in a scholarly and largely objective manner. She hopes to provide valuable insights for social historians. The account makes very interesting reading for those who desire a better understanding of the background, milieu, style of life, and attitudes of the Sri Lankan elite.

The author understands that she has to cope with blurred boundaries between fact and legend and has done her best to separate them. She seeks to illustrate the way of life and norms of the prominent Bandaranaike family. Her clan descends from a high official who served Ceylonese rulers before conquest of the island by the Portuguese, then the Dutch, and, finally, the British. Like other indigenous elites, her clan rapidly adopted the religion, behavior patterns, and norms of successive European rulers, most particularly those of Victorian England. Serving as officials and members of the judiciary of Ceylon under the Colonial Office, many of her Bandaranaike ancestors received distinguished favors and honors from the raj.

A number of her relatives went to Oxford, Cambridge, or the Inns of Law to round off their Anglicization. As owners of large estates, their wealth and position enabled them to emulate in detail the life style of the Victorian English gentry. In general they outdid themselves in loyalty to the crown. They remained, nonetheless, natives as far as their rulers were concerned. The interaction reveals fascinating ambiguities.

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Careful reading gives considerable insight into the complex world of the collaborator and mediator between white rulers and the indigenous population, the vast majority of whom were in every sense subordinate to and often exploited by the high-born indigenous elite. Insight is also given into the transition the elite had to make when independence finally came to Sri Lanka.

The volume also contains a useful index, a glossary of indigenous terms, and a complicated but helpful family tree. The book is a pleasure to read.

ROBERT I. CRANE
Syracuse University

UNITED STATES

CAROL ZISOWITZ STEARNS and PETER N. STEARNS. *Anger: The Struggle for Emotional Control in America's History*. Chicago: University of Chicago Press. 1986. Pp. vii, 295. $24.95.

The last decade has witnessed a remarkable outpouring of scholarship on anger. Physiologists have linked angry feelings to specific changes in hormonal activity, body chemistry, heart function, and brain activity. Cultural anthropologists have traced the societal determinants of the arousal and expression of anger, and social psychologists have interpreted anger as a social event and a method of communication.

Now, Carol Zisowitz Stearns and Peter N. Stearns show that a historical dimension can be injected into discussions of the expression and control of anger. In this sophisticated, intelligent, and thought-provoking book, they trace the ways that Americans have attempted to restrain, channel, and defuse the overt expression of anger over the past two centuries. Drawing on popular advice columns, popular fiction, and, to a lesser extent, autobiographical materials and surveys of opinion, they argue that the past two centuries have witnessed a concerted "campaign" to restrain anger. They conclude that, despite the proliferation of "get-the-anger-out, be-honest-with-yourself" therapies during the 1970s, Americans today are more intensely concerned with repressing the emotion than their ancestors were.

The authors divide the history of efforts to control anger into four stages, beginning with the American colonies in the early eighteenth century. Colonial society, they maintain, with a few exceptions, showed few signs of concern about anger in itself. Anger was freely expressed in the home, in law, and in religion and was not explicitly condemned, since it served important functions: the enforcement of social hierarchy and the regulation of social relations in a society based on concepts of honor and shame.

By the middle of the eighteenth century new religious and philosophic ideas had encouraged a heightened concern with emotional control within the family. Advice manuals now included specific injunctions against displays of anger. Diaries revealed a growing sense that anger needed to be struggled with and conquered. Experts on child rearing rejected physical discipline and substituted a new ideal of instilling a capacity for self-control into children through various forms of psychological influence. Writers on marriage warned that displays of anger, particularly by women, would ruin a marriage. The authors argue that the growing preoccupation with emotional control reflected a desire to create a compensatory sphere of order and harmony against the upheavals of a changing economy. They maintain that the new stress placed on domestic harmony encouraged expression of anger in disguised forms, especially among women.

By the middle of the nineteenth century a new approach to controlling anger appeared, as a growing number of parents sought to channel and harness anger, particularly in their sons. The new emphasis on channeling, apparent in a rage for athletic activity and a new willingness to tolerate limited displays of anger within marriage, reflected the intellectual temper of the late nineteenth century, the tough-minded sense of life as risk and struggle, as well as a belief that anger, when carefully channeled, was a useful economic and moral tool.

During the 1920s a new attitude toward anger began to take shape. By paying closer attention to supervisory training and employee morale, industrial psychologists, scientific managers, and human relations experts sought to reduce anger in the workplace in favor of smooth interpersonal relations. Experts on child rearing, although more tolerant than their predecessors of mild outbursts of anger, encouraged parents to emphasize the valuelessness of anger to their children. Marriage counselors attempted to teach spouses how to handle anger within marriage through "stage-managed" quarrels, "mock" fights, and sharing of feelings.

This important book will be of enormous interest to historians of the family, labor, and women. It is also sure to be controversial, in part because of its heavy reliance on a modernization framework and functionalist explanations of change. Future studies of anger will build on this book but will probably revise its interpretive framework. The book's picture of change is excessively unilinear and needs to be redrawn. Most problematic is the discussion of the "premodern" background, in which the authors argue without sufficient evidence that colonial Americans "felt relatively free to storm and rage when the mood seized them" (p. 2) and that communal supervision and formal social hierarchy

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served as an effective substitute for internalized self-restraints.

Further, the authors say almost nothing about countertrends. They ignore earlier efforts to legitimate the expression of anger (such as the early twentieth-century Emmanuel Movement), dismiss the assertiveness training and self-awareness therapies of the late 1960s and 1970s as an "emotionological blip" (p. 209), and minimize ethnic and class differences in handling anger.

The underlying causes of change also need further elucidation. Instead of invoking such abstract economic explanations as the rise of a managerial, service-sector economy, scholars should pay more attention to specific social developments—such as the decline of apprenticeship and fostering out of young children, falling birth rates, the increasing psychological and structural isolation of the home, and the increasing influx of mothers into the workforce—that intensified middle-class concern about anger. Finally, future studies of anger control will need to pay closer attention to temporal variations in what provokes anger and how it is expressed.

STEVEN MINTZ
University of Houston

ROBERT C. FULLER. *Americans and the Unconscious.* New York: Oxford University Press. 1986. Pp. 248. $19.95.

Historians have long stressed the interplay of religious and psychological interests and ideas and have, for example, situated the origins of American experimental psychology in post-Darwinian attempts to approach religious concerns from a scientific perspective. In this fine cultural and intellectual history Robert C. Fuller further explicates this relationship and delineates an American tradition of interest in unconscious mental phenomena that stretches from Jonathan Edwards's "holy affections" to Rollo May's "reality beyond the senses." In Fuller's view this line of development exhibits a continuing concern for an individual's "inner experience" of "aesthetic spirituality." This tendency to "symbolize harmony, restoration and revitalization" reflects America's "underlying cultural patterns" and stands in sharp contrast to European views of the unconscious. Fuller thus stresses the "theological anticipations" of eighteenth-century theologians and Ralph Waldo Emerson's Over-Soul, closely examines William James's resolution of his personal spiritual crisis, and devotes special attention to the recent emergence of humanistic psychology. He also sketches behaviorist attempts to define the unconscious as learned behaviors without verbal correlates, the reception and transformation of psychoanalysis in the United States, and such influential popular psychologies as mesmerism, spiritualism, theosophy, and New Thought.

Fuller writes best about those who explicitly intertwined religious and psychological concerns, and his analysis of *A Pluralistic Universe* and *Varieties of Religious Experience* well demonstrates James's debt to the tradition identified here. Similarly, Fuller insightfully discusses humanistic psychology as a "third force," in overt opposition to the determinism of both behaviorism and psychoanalysis. Humanistic psychologists such as May, Carl Rogers, and Abraham Maslow thus join Emerson and James as Fuller's heroes, and he sees their work as promoting further reconciliation of religious ideas and psychology. Although this analysis derives largely from Fuller's intellectual and institutional point of view as a professor of Religious Studies, it represents his most substantial historical achievement.

Despite these strengths Fuller's book is not definitive. He paints with broad strokes, explicitly omits important figures whose work paralleled that of those he discusses, and generally exhibits less attention to detail than many would prefer. More important, in stressing ideas and culture, Fuller downplays both social context and professional community. Not surprisingly, he remains uncomfortable with topics furthest from his focus, and his discussions of American experimental psychology and behaviorism contain small errors and misemphases. Nevertheless, historians will emphasize this volume's real accomplishments and view its relatively minor limitations as opportunities for their own research.

MICHAEL M. SOKAL
Worcester Polytechnic Institute

MICHAEL KAMMEN. *Spheres of Liberty: Changing Perceptions of Liberty in American Culture.* (Curti Lectures.) Madison: University of Wisconsin Press. 1986. Pp. xiv, 191. $19.50.

Michael Kammen has crafted a splendid souvenir (in the most literal sense) for the U.S. constitutional bicentennial. It is a triptych mosaic arranged in a sequential series of insights into the past and previsions into the future. These take the idea of liberty from British roots and American transplantation into a subsequent line of growth. In the process Kammen is both analyst and critic, providing a number of provocative, perhaps disturbing thoughts to the reader.

Possibly paramount is the implied thesis that the American constitutional construct and its separation of powers (already under criticism as anachronistic in a nuclear world) may well rest on a flawed base—Montesquieu's misunderstanding of the contemporaneous British political system and his failure to distinguish between formal constitutional and infor-

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mal political arrangements. Second is the proposition, amply proven, that "liberty" (though repeatedly used in the American Constitution) is an inherently undefined and undefinable word that takes content and meaning from counterpoised correlatives—liberty and order, liberty and property, liberty and justice. The final point is that the transition of the idea from freedom from mere physical restraints to unfettered moral choice of mind and psyche is no lineal progression but a succession of discontinuities. This assertion is borne out by Kammen's comment that the Supreme Court's *Adkins* decision of 1924 "seems to combine a Darwinian faith as relentless as Calvinism with a candor so lacking in guile that we feel centuries rather than decades might lie between us and this *ancien regime*" (p. 106).

Near the start of the twentieth century Lord Acton insisted that "liberty" had two hundred definitions. Contrariwise, Kammen has not only completed his towering project (regretfully omitting religious freedom) but has compacted vast reading and deep reflection into a few hundred pages. Moreover, in exploring the penumbra and nuances of an equivocal word, he has explicated its nuclear meaning. Borrowing from Governor John Winthrop in 1645 and Walter Lippmann in 1920, the author offers his reader insights gained not by cognition but by the instinct of encounter, or better, by the shock of loss.

As something of a coda, the book includes notes on the depiction of liberty in American iconography and, better yet, embellishes its text with illustrative depictions in painting, china, and textiles to suggest graphically the interplay between the driving political impulse and the artistic response in the American character. The author thereby vindicates Potter Stewart's analogous confession on pornography: that recognition comes from seeing, not definition.

GERALD T. DUNNE
St. Louis University

LEONARD W. LEVY, *et al.*, editors. *Encyclopedia of the American Constitution*. In four volumes. New York: Macmillan. 1986. Pp. xciv, 471; 473–1002; 1003–1618; 1619–2196. $320.00 the set.

The bicentennial of the Constitution of the United States is a momentous occasion. Already a chief justice of the U.S. Supreme Court has resigned to direct its celebration. Many books pertaining to American constitutional history are being published, but few will match the importance and massiveness of this four-volume work, which was subsidized in part by the National Endowment for the Humanities (NEH).

Leonard W. Levy (historian), Kenneth L. Karst (legal scholar), and Dennis J. Mahoney (political scientist) edited the encyclopedia, the first ever published on the Constitution. The editors note that "the Encyclopedia is intended as a scholarly and patriotic enterprise to commemorate the bicentennial" (vol. 1, p. vii). In addition, the editors sought to fill what they perceived to be the need for a comprehensive, multidisciplinary reference work on the Constitution.

The four volumes comprise approximately two thousand one hundred entries. Ranging from *Abington Township School District v. Schempp* (1963), a case concerning the constitutionality of a Pennsylvania law requiring the daily reading of at least ten verses of the Bible in public schools, to *Zurcher v. Stanford Daily* (1978), a case assessing the constitutionality of a search and seizure of evidence from Stanford University's student newspaper, the articles cover a vast spectrum of history and law. According to the editors, 55 percent of the entries discuss legal concepts and doctrine. The remaining 45 percent is divided among cases (15 percent), people, such as presidents or judges (15 percent), historical periods (10 percent), and statutes, treaties, or administrative laws (15 percent). Major articles have been allowed up to 6,000 words, but most of the essays are less wordy.

Substantively, the majority of the articles embrace what might be termed "traditional" political legal history. Less attention is given to economic and social implications in the Constitution's past. Moreover, the cases discussed have been chosen primarily from the modern era. Both of these emphases are justifiable, although Charles Beard and Mary Beard might have had something to say about the choices. Incidentally, Charles is the subject of a four-paragraph entry; Mary is not, but she is mentioned in the essay on Charles (vol. 1, pp. 104–05).

Some 262 authors contributed entries to the encyclopedia. The editors did not shirk their duties. Levy wrote 305 articles, Karst penned 295 articles, and Mahoney authored 259 articles. A previous assistant editor, David Gordon, wrote 187 entries. This constitutes nearly one-half of all the essays. A sixteen-member editorial board assisted the editor, and all but one, William J. Brennan, Jr., authored at least one selection.

The contributors include 41 historians, 53 political scientists, 135 law professors, and 29 attorneys and judges. This is, to historians, a surprising imbalance. It also reflects the substantive bias of the volumes. There are some very appropriate assignments: C. Vann Woodward on the Compromise of 1877, Eugene V. Rostow with war, foreign affairs, and the Constitution, Anthony Lewis on *Gideon v. Wainwright*, Robert H. Bork with judicial review and democracy, and Archibald Cox on the First Amend-

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ment. Many academic emeriti have also offered excellent essays.

Mindful of the concerns of a "democratized" NEH, the editors point out the geographical distribution of the authors. All regions, they say, are covered. This is true only in a general sense. Not every state has a contributor. A better distributive barometer might be found by discovering where the authors were educated, but this is beyond the scope of the four volumes and this review. Clearly, a strong tinge of southern California and New England characterizes the authors' collective backgrounds.

This is a most valuable reference work. It should be purchased for every library in the United States. It is very readable, it is clearly organized, and it offers much information about the Constitution. The editors are to be praised for accomplishing this arduous task.

JOHN R. WUNDER
Clemson University

WAYNE A. HUSS. *The Master Builders: A History of the Grand Lodge of Free and Accepted Masons of Pennsylvania.* Volume 1, *1731–1873*. Philadelphia: Grand Lodge F. and A.M. of Pennsylvania. 1986. Pp. xi, 327. $22.95.

In this study Wayne A. Huss traces the history of Pennsylvania Masonry from its colonial beginnings and early growth, through the disarray of the anti-Masonic period, to its recovery in the 1850s. By the end of his story, in 1871, the fraternity was flourishing as a highly respected and respectable organization. Huss aptly chronicles the details of administrative developments, organizational conflict, and building programs. He also provides extensive statistical material on membership; he notes, for example, that the period 1856–71 was similar to previous eras in that "Freemasonry in Pennsylvania appealed mostly to independent craftsmen, professionals, and men involved with commerce, as well as others who conducted business essentially within an urban or small town environment" (p. 229). Huss finds the relatively large percentage of artisans in the fraternity surprising because of the high cost of membership but offers no suggestions to explain this apparent anomaly.

Huss is sometimes critical of the fraternity, but generally his tone is one of praise for the accomplishments and ideals of Masonry. Masons, to whom the book seems directed, should find the study satisfying. Most historians, however, will be disappointed. Dorothy Ann Lipson's book, *Freemasonry in Federalist Connecticut, 1789–1835* (1977), set a fine example of how the study of Masonry could provide a window into the social, political, and religious milieu of early nineteenth-century America. In contrast, this study appears almost to have been written in a vacuum. Details of the anti-Masonic movement and Masonic response are provided with little attempt to analyze the underlying factors that made Pennsylvania ripe for anti-Masonry. For example, no analysis of religious conflict emerges here. Huss notes that the religious sentiments of Grand Lodge leaders changed through time, from deism, to Protestant Christianity, to a secular religion of "private morals and public virtue" (p. 164) devoid of theology, but does not examine these changes in the context of the broader society's religious developments. Similarly, he documents urban-rural conflict in the late Federalist period without exploring its implications. Particularly intriguing were rural Pennsylvania Masons, who in 1822 argued for a national Masonic constitution patterned after the U.S. government, which they hoped would diminish Philadelphia's control over Pennsylvania Masonry. This book, then, is tantalizing in that it hints at but does not explore rich material. On occasion, Huss refers the reader for further analysis to his dissertation, "Pennsylvania Freemasonry: An Intellectual and Social Analysis, 1727–1826" (1984). I suspect that historians will find that study, and not the book, enlightening.

LYNN DUMENIL
Claremont McKenna College

ROBERT A. ROCKAWAY. *The Jews of Detroit: From the Beginning, 1762–1914*. (Great Lakes Books.) Detroit: Wayne State University Press. 1986. Pp. xi, 162. $15.95.

Robert A. Rockaway's study of Detroit Jewry joins an increasing body of historical literature on American Jewish communities. The volume covers the history of the Detroit community from the first settlers in the mid-eighteenth century to the eve of World War I. Its primary focus, however, is the period from 1880 to 1914, when the older community of German Jews was overwhelmed by the mass migration from Eastern Europe. Only eight pages are devoted to the Jewish settlement before 1850. A second volume, now being prepared by another author, will trace the community's history from 1914 to the present.

All historians must face the question of determining their intended audience. Ideally, a Jewish communal history should maintain a balance between scholarly rigor and popular appeal by carefully weaving theoretical and analytical questions into the fabric of historical narrative. In this study, however, Rockaway has not attempted this admittedly difficult balancing act. As he explains in the preface, his purpose "is to acquaint the Detroit Jewish commu-

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nity of today with its 'roots'" (p. xi). The character of the book is underscored by its format—a modified "coffee-table" style, with extensive illustrations that occupy more space than the text on many pages. Rockaway has excised or condensed much of his analytic material, added a two-page conclusion, and reserved his socioeconomic and demographic data for an appendix. Although these charts and tables are interesting in themselves, he would have been better served by integrating this material into the text.

Rockaway has provided a readable and informed communal history with a wealth of wonderful illustrations and illuminating and instructive anecdotes. His account raises important questions for the study of American Jewish communities in general. He presents a picture of a Jewish community whose history is in part distinctive and in part similar to the histories of many other American Jewish communities. He fails, however, to explain why this is so. Why, for example, did Detroit Jewry experience a high level of overt anti-Semitism? Why did members of this same community nevertheless play an early and active role in local politics? What role did Jews play in the rapid industrialization Detroit experienced in the late nineteenth and early twentieth centuries? What part did they play, if any, in the development of the auto industry?

JUDITH E. ENDELMAN
Henry Ford Museum and Greenfield Village
Dearborn, Michigan

JAMES H. MADISON. *The Indiana Way: A State History.* Bloomington: Indiana University Press or Indiana Historical Society, Indianapolis. 1986. Pp. xvii, 361. $20.00.

This is an attractively ordered historical account of the state of Indiana, emphasizing components and uniqueness of the "Indiana way" of moderation and evolutionary change. Also emphasized is Hoosiers' perception that theirs is the most American of the states, a perception at times at odds with the historical reality James H. Madison describes.

State and local history does not always fit well into periodization constructed to accommodate national developments. Historians often avoid the problem by marking off decades, like census columns, to serve as convenient receptacles for information illustrating change or lack thereof. Here, however, an intelligent four-part structure divides prestatehood (to 1816) from the pioneer era (to 1850), a seventy-year "transition" era, and the years since 1920. This schema eliminates the staccato effect shorter time units produce; it focuses attention on longer-ranged trends in development. If the Indiana "way" of moderation and evolutionary change is unique, it should be demonstrable through comparison. This Madison does by evenly directing his attention to political, economic, social, and cultural developments and building arguments from a synthesis of monographs, dissertations, and essays. His history of education in Indiana demonstrates, sometimes in critical terms, Hoosiers' commitment to local control and distrust of modernity. Close partisan divisions in Indiana politics illustrate cause and consequence of commitment to moderation and slow change. Rates of industrialization, especially when compared with neighboring states, provide evidence of a clinging to older values and a preference for evolutionary alterations—the belief, as Madison puts it, "that God preferred Hoosiers behind a plow or at least in a vegetable garden" (p. 177). The 95 percent of Hoosiers in 1920 who were native born and the 97 percent who were white provide a significant base for explaining the state's conservatism. Sometimes, Madison does not cite authority for his assertions; an example is his claim that political patronage had "a longer and more durable history in Indiana than in nearly any other state" (p. 134).

Although the burden of the thesis requires interstate comparison, Madison is also careful to describe and account for the role of the cultural zones resulting from patterns of settlement in southern, central, and northern Indiana. Well-executed maps and charts supplement the explanations. This book's structure and comparative explanations make it an unusually attractive history. Indiana's "way" may border on blandness, but this account of its historical development is vigorous and compelling. Regardless of Hoosiers' self-perception of representativeness, Madison employs interesting illustrations of its uniqueness.

CARL UBBELOHDE
Case Western Reserve University

CHARLES C. BATES and JOHN F. FULLER. *America's Weather Warriors, 1814–1985.* College Station: Texas A&M University Press. 1986. Pp. xxiv, 360. $29.95.

Since the War of 1812 the military has made a long series of contributions to meteorology. Charles C. Bates and John F. Fuller have combined to offer this thorough and interesting book on American military weather personnel.

By far the most exciting portion describes the weather in World War II. Mobile weather personnel at the fronts, on ships and submarines, and in rear echelons planned for operations and, despite limitatons on forecasting, provided amazing accuracy.

After 1945 Navy, Air Force, and Army meteorologists turned to nuclear testing, the modification of weather, private meteorology, and forecasting se-

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vere weather. The Berlin Airlift and the undeclared war in Korea demanded massive assistance from meteorologists. The 1950s saw the introduction of computers and numerical forecasting, and in Korea forecasters now flew combat missions. Inexperience and deficient data minimized the impact of improved techniques during the struggle in Korea. Elsewhere, the horrors of nuclear fallout became apparent from persistent testing, in which military forecasters assisted, as they did at bases all over the world under the Strategic Air Command. The International Geophysical Year, weather satellites (TIROS), and hurricane reconnaissance were endeavors of benefit to civilians. The Vietnam struggle brought further urgent prognostications to enable more accurate bombing. Military commanders, however, sometimes ignored the weather personnel in demanding combat strikes.

The proliferation of duties continued. In 1980 the state of the art of forecasting prevented the military from predicting suspended dust in Iran, a major factor leading to the tragic failure of the attempt to rescue American hostages. Today, the Air Weather Service and the Naval Oceanography Command still pursue the goal of minimizing the effects of the environment on the military's operations.

The authors relate a fascinating story, but at times it is too much a catalogue of command nomenclature and names. The significant leaders are mentioned, but detailed accounts of the lives of these weatherpersons are too seldom included. More could be told about the military and the National Advisory Committee for Aeronautics, established in 1915. Poor transitions are at times made between subtopics. However, the authors concisely describe the military leaders who made a lasting impression on the development of meteorology in the United States and their intricate relationships with academic meteorologists as well as with the civilian Weather Service. For these reasons the book is a model addition to the study of weather forecasting and the collection of climatological records.

DONALD R. WHITNAH
University of Northern Iowa

BRUCE C. DANIELS, editor. *Power and Status: Officeholding in Colonial America*. Middletown, Conn.: Wesleyan University Press. 1986. Pp. xiv, 328. $35.00.

Who governed the colonies in English North America? How did the powerful acquire their authority? By what means were they able to sustain their power? Why did powerless colonists accept the dominion of their betters? Questions of this sort have informed the inquiries of colonialists for generations. Indeed, the responses to such questions have significantly contributed to the corpus of early American historiography. In short, studies of colonial elites abound. (To my knowledge, no one has as yet attempted to determine the exact number, but it is surely enormous.) Some, such as Charles S. Sydnor's *Gentlemen Freeholders: Political Practices in Washington's Virginia* (1952), have become classics of conception and interpretation. Fads come and go, but historians' fascination with colonists who ruled remains constant; the volume under review is but a recent testimony of the attraction.

Does this allure evidence a lack of imagination or an intellectual aridity in early Americanists? Perhaps, but less damning possibilities are assuredly more plausible. History records the uses and abuses of power through the ages. Accordingly, it is in the nature of the historian's craft to impart an ordered meaning to the past; necessarily, finding significance requires the study of those who wielded power. Further, posing and reposing the same questions does not always lead to the same conclusions. That archetypical expositor of nineteenth-century Whig history, George Bancroft, answered such questions in ways that now seem quaintly naive, if not altogether wrong-headed, especially if his pronouncements are gauged against the opinions of imperial or progressive historians. Compared to present scholarship, both imperial and progressive history lack sophistication and texture. The present book exemplifies the results that can be attained by pondering old questions anew and seeking answers by resorting to analytical methods and tools that colonialists must now command.

This book is a collection of eleven essays, all of which aim "to scratch a little deeper into the power structures of colonial America" (p. 5). To that purpose, Bruce C. Daniels, who edited the volume, and ten other authors scrutinized the elites, as well as the speakers of the lower houses of assembly, of Massachusetts, Connecticut, New York, New Jersey, Pennsylvania, Maryland, Virginia, and the Carolinas. The contributors have published on their chosen subjects elsewhere, but Lorena Walsh, Richard Ryerson, and Alan Tully are perhaps the group's best-known writers. Daniels remarks in the introduction that the authors relied on various research methodologies, although he asserts that "most of the essays are largely quantitative in approach and employ the innovative techniques of recent social history" (p. 5).

Covering the entire colonial era, the volume is divided into three sections. In the first, which the editor denominates "Local Leadership," Ronald K. Snell analyzes the justices in Hampshire County, Massachusetts, and their relationships to provincial and town government. Daniels's contribution is a study of officeholding patterns for selectmen in

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thirty towns in Connecticut; Walsh's investigation of the formation of the ruling elite in the Lower Western Shore of Maryland rounds out this section. Part 2, "Colony-Leadership," consists of five essays. Those of Tully and Ryerson concentrate on the role played by the Quaker party leaders in Pennsylvania politics, and A. Roger Ekrich's and Richard Waterhouse's essays describe the characteristics of the assemblymen from North and South Carolina. Thomas Wendel profiles 275 speakers from all thirteen colonies. Styled "Intercolonial-Leadership," whatever that means in context, the final section comprises articles by Jessica Kross, Thomas L. Purvis, and Grace L. Chickering: Kross examines the members of the New York Council of State; Purvis and Chickering investigate the councilors from New Jersey and the Old Dominion respectively.

All of the essays are informative after a fashion. They conveniently bring together a range of data that deepens our understanding of colonial leadership. The results of the obviously painstaking research are absorbing. And yet, I must confess I came away from my reading of this book not altogether satisfied by the experience.

The book's content is inconsistent because the contributions are not of uniform quality. Skill and insight vary among the individual authors. The essay by Walsh matches the quality of her earlier work. Those of Ryerson and Tully stand out because of what their authors have to say and how it is said. Both writers draw attention to the continuing vitality of the Quaker party after 1756, when the Quakers supposedly withdrew from politics; Ryerson and Tully thus provide a corrective to traditional interpretations of the Friends' involvement in politics. More importantly, they also imply a foreshadowing of modern parties and styles that is not normally associated with colonial politics, suggesting that political practices in Pennsylvania before 1776 may tell us more about the American party system than do those of other colonies. Kross's and Purvis's contributions are compelling because they treat long-neglected topics in useful, revealing ways. They too should remind us of the need to be more attentive to developments in the middle colonies.

The remaining essays are less satisfying. Wendel's reads more like a summary of his research notes than a sustained, cogent presentation of the evidence, and his conclusions are obvious to any well-read student. Chickering treats a familiar subject but in a muddled, if not altogether misinformed, fashion. Moreover, the amount of manuscript research, at least as revealed by the annotations, is unremarkable. Ekrich's piece does not compare favorably with his other writings. The contributions by Warterhouse and Snell are competent, but not grippingly presented. Daniels is occasionally "surprised" at his findings. What is startling or, for that matter, revealing about "the date of settlement affecting the pattern of selectmen's officeholding" (p. 49)? What is the meaning of his phrase "urban towns"? That bit of social scientific jargon is but one of many graceless neologisms and phrases evident in these writers' vocabularies.

Lacking a thematic focus, the pieces are not united into a coherent book. On this score, Daniels, as editor, would surely have profited from contemplating Thad W. Tate's introductory essay in Tate and David W. Ammerman, *The Chesapeake in the Seventeenth Century: Essays on Anglo-American Society* (1979). Surveying earlier Chesapeake scholarship, Tate provided a context for new work that constituted the bulk of the volume. Thereby, he gave his readers a sense of direction. Daniels's introduction provides none of that, and as a result the book is flawed from its outset. A knowledgeable reader will be put off by the editor's apparent unwillingness to acknowledge the existence of earlier scholarship as well as his propensity for inflating the book's importance; a neophyte will be both confused and led astray. Either way, the work of the contributors is badly served, and the worth of the collection is diminished.

WARREN M. BILLINGS
University of New Orleans

YASUHIDE KAWASHIMA. *Puritan Justice and the Indian: White Man's Law in Massachusetts, 1630–1763*. Middletown, Conn.: Wesleyan University Press; distributed by Harper and Row, Scranton, Pa. 1986. Pp. xiv, 335. $35.00.

Yasuhide Kawashima joins a growing number of scholars who have begun to ask not only how Anglo-Americans used and adapted their legal tradition to subjugate Indians but how the natives responded to the newcomers' law. Having combed most of the surviving judicial and legislative records, both colony and county, he provides a far clearer picture than we have ever had of how Algonquian natives fared in Massachusetts courts. Although Indians were accorded the same procedural rights as English, their treatment was in fact unequal, particularly in criminal cases. Compared to whites, their counsel was less effective, they had less stature as witnesses, and, when sentenced, they more often had to sell themselves into servitude to work off fines. The few Indian jurors and even fewer native magistrates served only to try other Indians. Although natives and whites committed the same crimes, the colonists' irrational fear of Indians and their efforts to use the law to exact social conformity often resulted in harsher penalties for natives.

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Kawashima's considerable accomplishments are blunted by his becoming mired in constructs and questions that are inappropriate for his purposes. He maintains that Massachusetts initially sought to assimilate natives and only gradually shifted toward treating them as a permanent, inferior minority, increasingly similar to blacks. But his text, organized around legal categories, jumps around in time and space, undermining the coherence of his most compelling argument. And, despite showing patterns of discriminatory treatment, he strives to demonstrate that the colony sought to be "fair" to Indians, especially in acquiring land. But his criteria for "fairness" are those of earlier historians rather than those of either Indians or English in the colonial period.

Kawashima also fails to render the natives' perspective on their legal relations with the colonists. He asserts that Indian justice was rooted in the "blood feud" and "clan responsibility" but derives these vague generalizations from secondary materials that are mostly unconcerned with New England Algonquians. His attempt to explain Indian concepts of land use and "ownership" does not draw on William Cronon's more lucid and informed discussion (*Changes in the Land: Indians, Colonists, and the Ecology of New England* [1983]). He ignores the notions of reciprocity that underlay native participation in trade, diplomacy, and even many land cessions. When Indian magistrates appear to have acted arbitrarily (p. 31) or when native sellers of land returned to the fee-simple purchasers for additional payments (p. 59), he tells us only that they did not understand English law and does not look for the values underlying such behavior. A pattern of avoidance of the courts (except, of course, as defendants) should suggest that, far from having lost their culture, Indians were still resolving a wide range of "legal" questions in their own way.

Kawashima makes clear the potential of court records for enhancing our understanding of Indian-white relations and of colonial law. But, when used in combination with the full range of documentary sources and ethnohistorical scholarship, these records can tell us far more than he suggests about the natives themselves. The full promise of Indian legal history for our understanding of colonial New England has yet to be demonstrated.

NEAL SALISBURY
Smith College

HARRY S. STOUT. *The New England Soul: Preaching and Religious Culture in Colonial New England.* New York: Oxford University Press. 1986. Pp. xii, 398. $29.95.

During one of the opening engagements of the Revolutionary War Ethan Allen led several score New Englanders in a surprise attack on Fort Ticonderoga. His men slipped into the fort at dawn, and when the British commander appeared in his nightshirt, he faced a row of American rifles aimed at his chest. In good British fashion the commander demanded to know by whose authority Allen and his men had entered the fort. Allen might simply have pointed at the rifles, but like other New Englanders he was eager to prove the legitimacy of his cause. His authorities, he said, were "the great Jehovah and the Continental Congress." The Continental Congress was, of course, a new and untried institution, but Jehovah was a name to conjure with.

For the past two decades colonial American historians have argued about the role of God in Puritan society. Did New Englanders have a special relationship to "Jehovah" as Allen implied? Or was religion for them merely a weekly interruption in their predominantly secular lives? Departing from Perry Miller's thesis that New England's early history could be strung on "the thread of an idea," social historians have used vital statistics, account books, and other secular records to rewrite New England's history. And a new generation of religious historians has examined the clergy and their sermons, asking whether Puritanism was a "popular culture."

Harry S. Stout's book draws extensively on the work of these social and religious historians yet constructs a synthesis that is surprisingly congenial to Miller's portrait of the New England "mind." Like Miller, Stout argues that religious ideas pervaded all levels of Puritan society and shaped "cultural values, meanings, and a sense of corporate purpose" (p. 3). Like Miller, he sees the American revolution in the Puritan colonies as an expression of faith, directed toward "the preservation of the gospel and New England's ongoing covenant with God" (p. 271).

Stout gives substance to these familiar ideas with some unfamiliar sources; during almost a decade of research he read several thousand manuscript sermons. He contends that published sermons tend to exaggerate the changes in Puritanism over the years. Occasioned by particular religious or political events, they focus on changing circumstances and so obscure the fact that every week New Englanders listened to sermons—about seven thousand in a lifetime—that reiterated the fundamental beliefs of the Puritan forefathers.

Although supporting Miller's claim for the pervasiveness of Puritan experience, Stout's picture of "preaching and religious culture" in colonial New England challenges some of Miller's assumptions. The rise of religious liberalism during the 1700s, for example, seems less a break from the past to Stout than to Miller. Stout shows that in weekly services religious liberals often preached traditional sermons on conversion: "Anglicization, in short, gilded the

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face of New England Society, but did not transform its soul" (p. 128).

In some cultural histories disembodied ideas seem to talk to other disembodied ideas, and one misses the flesh and blood of real human beings. Miller gave pungency to his work by developing one of the most colorful styles in American historical writing. Stout does not have Miller's flair for words—indeed, who does? But his account is well written and benefits from an abundance of good quotations, some from manuscripts that may not have been read for two hundred years until Stout's researches. In a typical passage Stout tells us that in 1766 a young pastor named Perez Fobes asked his parishioners to "suppose while you have been rambling in the Fields, you should espy at a small Distance, a Number of Stout Men, furnished with . . . a large fishing net, and seemingly engaged in fishing for the wind." You would think the men "distracted," Fobes said, and so was any man who should go "fishing" for salvation without the gospel (p. 269). The pithy imagery of this sermon and its emphasis on conversion remind the reader of sixteenth-century sermons by John Dod or of preaching a century later by John Cotton.

Not the least of Stout's achievements is that he brings such passages to light. They add flavor to his account and give support to his claim that many New Englanders worshiped "the great Jehovah" in 1776 in much the same way as their forefathers had in 1630.

J. WILLIAM T. YOUNGS
Eastern Washington University

TOBY L. DITZ. *Property and Kinship: Inheritance in Early Connecticut, 1750–1820*. Princeton: Princeton University Press. 1986. Pp. xvi, 213. $32.50.

With this meticulous study Toby L. Ditz joins a major debate on early American history. Were rural Americans in the North individualistic profit seekers, or were they oriented to local social relations and markets? To arrive at an answer, Ditz uses a carefully constructed analysis of inheritance in two areas of Connecticut that had different patterns of economic development.

Ditz compares four upland towns with one in the Connecticut Valley. The upland towns had poor soil, low surplus, and extensive agriculture in the mid-eighteenth century. By the early nineteenth century these communities had turned to rural industries to supplement their farms. From its settlement the valley town of Wethersfield had the best farming land in the colony; by 1820 it was a commercial farm community.

Both areas began with similar inheritance strategies, which Ditz identifies as "extended cognate," in which sons received unencumbered property. Under this system families attempted to establish a landed base for as many children as possible, with a "favored heir plus burdens" pattern that gave all siblings a claim to the family's main holdings. To be sure, these were not equal claims. Inheritance was asymmetric with preferential partibility. Although land remained a male preserve, as Ditz says, "most colonial families short-changed some sons" (p. 71). Daughters (and widows) had secondary claims on core holdings and only received small fragments of real estate when personal property did not suffice to meet their secondary claims.

By 1820 the two areas had diverged in inheritance practices. The uplands maintained the earlier pattern, but in commercialized Wethersfield poorer farm families now gave no land to daughters whereas in wealthier farm families daughters received as much land as did sons. Wethersfield sons now received almost twice as much personal property as daughters. Upland widows received the same treatment as previously, but widows in valley towns also received larger shares of family property for their maintenance. The consequence of these changes for women were contradictory: they had more land, but it meant less.

The clear research strategy and the careful discussion of the interaction of property and kinship make this the most helpful study to appear on the subject. But Ditz leaves unanswered a host of questions about these small communities and the members of their households. The upland towns engaged in dairying, and yet this did not develop into commercial dairying. Since this was an important means of rural prosperity in other areas, one wonders why. Ditz gives little sense of what women or men did on these farms and therefore few clues about the infrastructure of labor and kin relationships. Without more evidence this study cannot take us beyond the principal debate into new territory. And that is where rural studies need to go.

JOAN M. JENSEN
New Mexico State University

THOMAS P. SLAUGHTER. *The Whiskey Rebellion: Frontier Epilogue to the American Revolution*. New York: Oxford University Press. 1986. Pp. 291. $19.95.

Thomas P. Slaughter assumes that we cannot understand "the drama of early American politics" unless we push "the frontier to center stage . . . where contemporaries knew but historians have forgotten that it belongs" (p. 6). He asserts that "conflict was at the heart of the Revolution" and that the story of that "conflict among Americans was at least as important a part of the story as cooperation against the common enemy" (p. 4). This book fo-

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cuses on the conflict between the frontier and the more settled regions, between periphery and core, between localists and cosmopolitans, and between those espousing court and country ideologies in the revolutionary republic. The issues behind the conflict were taxation and control: the westerners, on the one hand, protested that a distant government could not represent them and, convinced of the east's insensitivity to their interests, employed violence or the threat of secession to resist efforts of the central government to assert its authority. The centralizers, on the other hand, had an economic interest in maintaining control over the west and also thought that the alternative to doing so was "anarchy and confusion" (pp. 44–45).

Slaughter claims to be equally sympathetic to both sides. "Neither perspective was 'right,' and neither was 'wrong.' Each made sense given the experiences, interests, and ideology of its adherents" (p. 45). But his treatment of the protagonists is far from even-handed. Instead, he portrays those living in the frontier areas as heirs to the revolutionary tradition of no taxation without representation, whereas those who repressed them were guilty of repudiating their revolutionary heritage. They acted inconsistently because of their political and especially their economic interest in controlling the west. Slaughter reasserts more strongly than anyone else I have recently read a Beardian connection "between pecuniary self-interest and political action" (p. 225). East struggles with west for the same reasons that capital struggles with labor in the Marxian scheme: the nature of the protagonists makes conflict between the regions as inevitable as conflict between the classes.

Although this formulation evidently satisfies Slaughter, it leads to major blindspots in his account. For instance, in discussing developments in the mid-1780s he does not explore why John Jay was so willing to trade the right to navigate the Mississippi for commercial concessions in the Spanish empire, perhaps because he is unaware of the enormous benefits such commerce had conferred on the nation's economy during the last years of the Revolutionary War. Nor does he consider that Massachusetts creditors could have been pursuing more transcendent goals than their immediate self-interest when they embarked on the heroic policy of direct taxation that led to Shays's Rebellion. They could not have been entirely unaware, after all, that they risked some form of resistance by doing so. And in discussing developments in the 1790s he neglects to explore fully the "several obvious considerations" that Alexander Hamilton had in mind in preferring to test "the mettle of national law enforcement on the Pennsylvania frontier rather than in the Carolinas" (p. 120), where the excise was also resisted. References in Hamilton's papers to western Pennsylvania benefiting from military contracting are omitted from Slaughter's account.

Perhaps the most grotesque example of the distortion that his handling of the evidence leads to occurs at the conclusion of the book where he quotes from a letter George Washington wrote to Jay in November 1794 that refers to the repression of the Whiskey Rebellion as "having happened" at a "fortunate" time (p. 225). Anyone familiar with the historical context of this letter will realize that Washington was alluding to his hope that the successful assertion of the government's authority would have a good effect on Jay's ticklish negotiations with the British. Slaughter refers to the government's international concerns in repressing the rebellion, but, because he focuses so much on the frontier, he gives the impression that these only involved foreign sponsorship of secession schemes (p. 204). The reference to "fortunate" timing is instead used by Slaughter to elaborate an idea developed in an earlier chapter that Washington's policies toward the west were primarily motivated by his interest in its lands. In this chapter Washington is portrayed as a rapacious speculator, and the reader is informed that he "lied about the openness of his accumulation of [land] rights" (pp. 83–84). Although Slaughter acknowledges that in the 1780s "the threat of national disintegration . . . weighed more heavily on his mind" than did "the profit of investors" (p. 87), in his conclusion he seems to discount the possibility that Washington might also have felt more concern about resolving the nation's differences with Britain than about his real-estate investments.

Slaughter's refusal on the above mentioned occasion to acknowledge the manifest meaning of the words he quotes is symptomatic of a larger problem in his approach to evidence and raises serious questions about his trustworthiness as a guide to understanding his material. The picture this book presents of the Whiskey Rebellion is a distorted, sometimes inconsistent, and often confusing one whose principal effect is to challenge the legitimacy of the founding fathers' efforts to stabilize the republic. Given current fashions in the academy, it will no doubt have its partisans. But it does not seem to me to contribute very much to an understanding of the past.

RICHARD BUEL, JR.
Wesleyan University

TERRY D. BILHARTZ. *Urban Religion and the Second Great Awakening: Church and Society in Early National Baltimore*. Cranbury, N.J.: Fairleigh Dickinson University Press, or Associated University Presses, Toronto. 1986. Pp. 236. $27.50.

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Terry D. Bilhartz's slim volume raises as many questions about the Second Great Awakening as it answers by changing the focus from the rural South and western New York to Baltimore. Despite interesting findings and interpretive comments, the book lacks a cohesive interpretive thread.

Between 1790 and 1830 Baltimore experienced both rapid growth and religious transformation. In 1790 the church-going half of Baltimore's population belonged to a variety of denominations. Episcopalians had the largest following (12 percent), and affiliation cut across class lines. Forty years later churches served distinct classes and races. Methodists now ranked first (17 percent), and Catholics second. All denominations experienced numerical growth, with the Episcopalians and Presbyterians matching the pace of city growth. Foreign-language congregations and congregations doctrinally removed from mainstream orthodoxy grew the least.

Bilhartz argues that Methodists, Presbyterians, and low-church Episcopalians actively sought and institutionalized revival. Methodists chose camp meeting locations within easy travel of Baltimore, and weekend attendants often continued the revivals in their city congregations during the week.

Despite denominational variations, Baltimore's churches shared problems and endeavors. Rivalries between clergy and controversies over polity divided congregations. The Episcopalians, Methodists, Quakers, and Presbyterians faced schisms. The Roman Catholics, Baptists, and Lutherans had internal squabbles. Churches cooperated on reform, the Sunday school movement, Bible societies, and defenses of orthodoxy against the Unitarians, deists, and Swedenborgians.

As early as 1790 women formed the majority of congregations. They raised money and joined the new voluntary societies that came to characterize churches after 1800. In fact, Bilhartz suggests that the absence of men led church leaders to complain about the low state of religion in the 1790s. Although the revivals reached more women than men, the proportion of men was relatively high, thus giving awakening churches more equal sex ratios by 1830.

The book's statistical analysis of members (defined as male heads of families attending church services) is revealing but distorting, given that women made up more than three-quarters of active members. It does not differentiate between families in which both spouses were members and those with only one (usually female) member. The sample's bias toward married couples also has socioeconomic and gender implications.

The book is thought provoking in arguing that the awakening increased religiosity in all denominations, in denying class and social factors a causal role, and in seeing the awakening as urban and reactive to feminization, but these points are not developed. The author notes different levels of autonomy among women's benevolent groups and describes regulation of members' behavior and Christian education but does not search for gender patterns. Others will have to explore the areas Bilhartz has identified.

JOAN R. GUNDERSEN
St. Olaf College

ANNE NORTON. *Alternative Americas: A Reading of Antebellum Political Culture*. Chicago: University of Chicago Press. 1986. Pp. ix, 363. $24.95.

The "alternative Americas" alluded to in the title of this essay in cultural criticism are a Pilgrim North and a Cavalier South, so different in their cultures as to make a civil war inevitable. In Anne Norton's view, the Pilgrims were the descendents of Oliver Cromwell; consequently, they were authoritarian, patriarchal, repressive, and legalistic. After the revolution northerners turned their back on republicanism and attempted to create a state that was hierarchical, indeed aristocratic, by crushing the "pre-political" family and subordinating workers and women to the factory, which formed the model for the northern state. Northern industrialists sought to extend their dominion through social reform movements (including abolition), Indian removal (of which John Quincy Adams was the chief exponent), westward expansion, and, finally, the Civil War. Southerners, as heirs to the Cavalier-democrats, based their polity on the family, which embraced their slaves (who never rebelled "because they lacked . . . that consciousness of collective grievance and common identity which animates rebellion" [p. 172]). Because these pagan planters were agriculturalists, they identified themselves with provident nature and, hence, with women, as well as with blacks and Indians, from whom "the first families of Virginia derived their aristocratic pretensions" by virtue of their descent from Pocahontas (p. 182). Southerners opposed territorial expansion because it "meant expansion of the powers and machinery of government," including a standing army, all of which the North favored (p. 186). Indeed, "Southern politicians had decried 'the military-industrial complex' (in these very words) [no source is cited, however] since the war of 1812" (p. 251).

Most historians will find such opinions astonishing. They are, by and large, undocumented. The sources seem primarily to be several secondary works, some issues of the *Southern Literary Messenger*, occasional political speeches, several collections of poetry, and *I'll Take My Stand*, which Norton regards as "an accurate characterization of antebellum Southern political culture" (p. 99). The documenta-

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tion that is provided is irregular, incomplete, and often inaccurate. No sources are given for a number of anecdotes and quotations, and what are represented as direct quotations are often, in truth, paraphrasings. Portions of Lincoln's First Inaugural and his Lyceum and Gettysburg addresses are misrendered, and a comment attributed to Jefferson is fabricated. Such shoddiness is inexcusable in a work that purports to be a close reading of key texts.

JAN LEWIS
Rutgers University, Newark

DALE T. KNOBEL. *Paddy and the Republic: Ethnicity and Nationality in Antebellum America*. Middletown, Conn.: Wesleyan University Press; distributed by Harper and Row, Scranton, Pa. 1986. Pp. xx, 251. $25.95.

Dale T. Knobel has attempted to do for the Irish what Winthrop Jordan did for Afro-Americans in *White over Black* (1968) and Robert Berkhofer in *The White Man's Indian* (1978) did for Native Americans. He has explored the American mind through the medium of some other, alien group in its midst. Knobel chose Irish immigrants of the mid-nineteenth century for that exploration. Just as Jordan's opus was not about blacks per se, and Berkhofer did not analyze Indians, so too Knobel's contribution here is in what he has written, not about the Irish, but about Americans.

Americans developed the image, the word picture, the stereotype of "Paddy" as a vehicle for understanding themselves. Thus, the central questions posed here are how and why did particular images of the Irish evolve in the early decades of the nineteenth century, and what were the social consequences of those words for both the Irish and the Americans?

Knobel uses a specific methodology, content analysis, to answer his questions. Having culled an impressive array of nineteenth-century school books, magazines, novels, political speeches, plays, newspapers, and the like, he extracts the words used to describe the Irish. Concerned with both frequency and quantity, Knobel places them in fifteen categories, in both positive and negative contexts.

The image of Paddy did not remain stationary but shifted drastically within the American "conversation." In the period 1820–44, the chief flaws of the Irish were considered to have been produced by their flawed environment. American republican optimism held out the possibility that in a republican climate those flaws would be fixed. But after the 1840s the character of "Paddy" increasingly came to be seen as unchangeable. Racial in origin, his traits were fixed, and neither education nor republicanism could alter them. According to the author, this fluidity of image did not evolve from changing American social and economic conditions but from shifts in definitions of the American character.

Although the book could more sharply account for the transition from one "Paddy" to the next, it shows how historians might use content analysis to understand the "mind" of the past. This work also replaces the vagueness of previous studies of prejudice and stereotype against the Irish with precision and detail.

HASIA R. DINER
University of Maryland

MARJORIE B. COHN. *Francis Calley Gray and Art Collecting for America*. Cambridge: Harvard University Art Museums, distributed by Harvard University Press, Cambridge. 1986. Pp. xi, 344. $14.95.

Francis Calley Gray (1790–1856) was one of six children born to William Gray and Elizabeth Gray of Salem, Massachusetts, perhaps the wealthiest family of the new American nation and one whose wealth accrued from a network of shipping, trading, and financial underwriting of worldwide commerce. This interesting and often absorbing book, however, is less a study of familial wealth or entrepreneurial activities than a suggestive account of an intellectual who devoted his life to artistic, scientific, and social causes. Indeed, Francis Calley Gray was a cultural nationalist. And, although Marjorie B. Cohn's focus is on Gray the collector and connoisseur of what would be the core of the print collection at Harvard's Fogg Art Museum, Gray is hailed as a booster of American cultural institutions and a framer of artistic and educational ideals. The theme of promoting culture for America provides a fascinating perspective of a man's and his nation's search for artistic place.

Gray was as reflective as he was earnest. He was a rebellious Harvard undergraduate, involved in the student disorders of 1806 and 1807 and apparently at odds with everything from an inflexible curriculum to squalid living quarters and an archaic disciplinary code. Suspended from the college for three months, he was readmitted and graduated with notable academic achievements, including membership in Phi Beta Kappa.

A young man of talent and wealth, Gray enjoyed expansive opportunities. He served for two years in St. Petersburg as an assistant to John Quincy Adams, ambassador to the imperial Russian court. He loved foreign travel, which he pursued throughout his life. Admitted to the bar in 1814, Gray primarily looked after his father's business. Mostly, however, his energies were given to social causes or intellectual interests. He was elected to the Board of

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Overseers of Harvard University, and he served the Boston Athenaeum and the Massachusetts Historical Society, as well as both houses of the Massachusetts General Court. He gave generously of himself to a variety of reform movements but especially to the problems of prisons and convict rehabilitation.

As a gentleman scholar he was fascinated by natural history. Gray believed that a systematic study of minerals, fossils, and seashells could help reveal the world's history. Ultimately, he sought to preserve a history of art for the education of generations of Americans through his collection of prints. Cohn's monograph deserves a wide readership. Her book helps explain the legacy of Francis Calley Gray and his contributions to the culture of antebellum America.

RICHARD KLAYMAN
Bunker Hill Community College

ROBERT D. RICHARDSON, JR. *Henry David Thoreau: A Life of the Mind.* Berkeley and Los Angeles: University of California Press. 1986. Pp. x, 455.

"One world at a time" is a fitting theme for Henry David Thoreau's life. Often quoted as among the last coherent statements uttered before he died, it describes the consistency of his career as an observer, speaker, reader, and writer. His contemporaries and many subsequent writers have portrayed him as an idler, brilliant perhaps but easily distracted from serious writing by observing tree rings or toad spawn. Robert D. Richardson, Jr.'s, biography underlines the consistent path of his thought and the expanding dimension of his mind as it grasped and enlarged the ideas of his times.

The author demonstrates a thorough knowledge of the events and social climate in America during the lifetime of Thoreau. This work, divided into nine "books" with subtitles, begins in 1837 with the section entitled "Home from College." Richardson shows the influence of the Concord milieu of Ralph Waldo Emerson, Margaret Fuller, and Bronson Alcott on the young man fresh from Harvard. He expanded his college background in this atmosphere. Harbingers of the Thoreau "to be" can be seen in his study of German philosophy and the classics and in his first attempts at lecturing. The life of the mind unfolding is the core of this book. His early journals, his reading of Goethe, his attitude toward women, his distant affair with Ellen Sewall—all give the reader an insight into Thoreau's mind. Premonitions of things to come appear in Thoreau's early letters and journal. The author brings all of this out in a most fascinating and sometimes humorous way. The writing is skillful and sometimes brilliant.

Thoreau's early contact with the club founded by Frederick H. Hedge and nourished by Emerson and Fuller gave him his Transcendental roots. His article for the first issue of *Dial* is noted by the author. The author states that, by 1840 "self discovery, self conquest, interior immigration . . . for much of the rest of Thoreau's life . . . The landscape lies fair within" (p. 78).

Exploring the developing "inner landscape" is a description of Thoreau's intellectual biography. By studying Thoreau's journal, his letters, and those of his contemporaries, Richardson traces his mental progress from 1837 to 1862. This covers a mighty sweep of American history, not in years but in intellectual ferment. Brook Farm, the German higher criticism, Indian philosophy, the writings of Thomas Carlyle and Herman Melville, economic theories of J. D. Say and Adam Smith all helped create the mental climate that influenced Thoreau. As the author points out, he became a Stoic, a follower of the original Stoics, not of Seneca or Marcus Aurelius. Thoreau became more Apollonian and less Christian as time went on.

In "Wilderness and Society," on the years between 1843 and 1851, Richardson traces Thoreau's road to Walden, his naturalism, his study of nature to find human truth to recast ancient fables into modern mythical form. Richardson follows Thoreau's mind through the Mexican War, the John Brown episode, the Anthony Burns affair, and, finally, the onset of the Civil War. These events are in the background, yet the life of his mind is in the foreground influenced by American and European writers. Richardson, by collating Thoreau's readings from his withdrawals from Harvard library and the new books he received, is able to relate these readings to his essays, lectures, and, most of all, to *Walden* and thus provide fresh insight into this classic. For example, the author plays down the solar-seasonal structure of *Walden* and puts Thoreau in the center of the book, which goes beyond nature.

Despite the stereotype, Thoreau was not a recluse. He traveled enough in America to meet leading intellectuals such as Horace Greeley, Henry Ward Beecher, Walt Whitman, Thomas Wentworth Higginson, and Louis Agassiz. He disagreed with Agassiz on special creation and was ready to accept Darwin's findings when *Origin of the Species* came out. Of course, the major figures of the New England illuminati were known to Thoreau either as friends or acquaintances. Richardson brings out Thoreau's ever-expanding mind and his apparent plans for a major work in pure science after *Walden*. Perhaps that this was cut short by his death in 1862 is just as well.

Readers who have poured over Thoreau's journal, his letters, and other major works of his time no doubt will gain new insight from this biography.

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Richardson has achieved a major feat of intellectual history by putting it all together in "a life of a mind."

J. WADE CARUTHERS
EMERITUS
Southern Connecticut State University

HAROLD N. COOLEDGE, JR. *Samuel Sloan: Architect of Philadelphia, 1815–1884*. Philadelphia: University of Pennsylvania Press. 1986. Pp. xii, 262. $25.00.

Samuel Sloan first came to national prominence as an architect in the early 1850s, becoming well known both for his many commissions for churches, schoolhouses, and hospitals and for his first influential pattern book, *The Model Architect* (1851–53), which eventually went through five editions. Trained as a carpenter-craftsman, he always viewed what would become the profession of architecture as, above all, something that supplied a special service related to highly specific needs. He was much less concerned with aesthetics than with utility and felt strongly that American buildings must relate to American nature, materials, and conditions, much in the tradition of Ralph Waldo Emerson and Horatio Greenough. Sloan's architectural perspective was functional and nationalistic—well attuned, the author believes, to mid-nineteenth-century Jacksonian attitudes.

Five other popular Sloan pattern books later appeared, along with pamphlets, articles, and *The Architectural Review and American Builder's Journal*, which he edited, all helping to publicize his building designs and his architectural concepts. Working both alone and with various partners, he turned out a substantial body of work, including over twenty churches, thirty-two hospitals for the insane, more than fifty private and public schoolhouses, many public and commercial buildings, and countless private residences. Sloan importantly contributed to the concept of modular repetitive construction for institutional building and initiated improvements in fire safety, mechanical equipment, and internal circulation in public and commercial buildings. His writings and buildings had considerable influence on pre–Civil War American building design. In the post–Civil War period he clung to the ideas he had long espoused, however, continuing until late in life adamantly to reject European fashions and architectural cosmopolitanism. The best-known of his buildings is Longwood, an octagonal villa in Natchez, Mississippi, taken from a design in *The Model Architect* and presently designated a National Historic Landmark.

Although pedestrian in style and not convincingly portraying Sloan in a multidimensional way, Harold N. Cooledge, Jr.'s, biography provides a wealth of factual information, gleaned from exhaustive research in and careful analysis of often obscure sources. With text occupying less than one-half of the book, the work includes a highly informative listing of 171 confirmed Sloan commissions and 32 commissions attributed to Sloan, a bibliography of Sloan's publications, and a genealogical appendix. Over thirty years in preparation, Cooledge's definitive study is an important addition to the growing library of scholarly studies in American architectural history.

PAUL R. BAKER
New York University

JOHN HARLEY WARNER. *The Therapeutic Perspective: Medical Practice, Knowledge, and Identity in America, 1820–1885*. Cambridge: Harvard University Press. 1986. Pp. x, 367. $32.50.

Although therapeutics has always been crucial to the physician as well as the patient, historians have hesitated to focus their attention on its history. Most historians who have touched on the history of nineteenth-century American therapeutics have been content to describe the major changes in broad strokes, from bloodletting and the use of calomel and other purgatives in the antebellum period, to the use of whiskey, opium, and quinine in the post–Civil War period. In addition, most medical historians have relied on subjective sources, such as medical textbooks and articles in medical journals.

In this book John Harley Warner not only studies the history of therapeutics but also very successfully examines the "process of change." He has used manuscript sources from more than sixty repositories, and he has used the computer to analyze medical practice at the Massachusetts General Hospital and the Commercial Hospital of Cincinnati. After precisely determining changes in therapeutics over time, the author relates those changes to the intellectual history of the period. He describes early nineteenth-century therapeutics as based on Enlightenment rationalism with focus on systems of medical practice. Beginning in the 1820s, some physicians who had studied in Paris tried to reshape therapeutics by using a skeptical medical empiricism. The result was a conflict between the advocates of "heroic medicine" and those who sought to eliminate bloodletting and purging from the medical profession. By the 1860s a new factor entered the scene; the successful application of laboratory science encouraged physicians to argue against empiricism and instead to rely on what they considered a more solid foundation based on what could be measured, based on "science." The result was a "new rationalism" that transformed medical practice and in the process enhanced the role of the physician.

The new medical practice was based on "scientific" measures of normality. Unlike their predeces-

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sors, late nineteenth- and early twentieth-century physicians were not concerned with the question of whether one was called an allopath or a homeopath but instead with whether the practitioner had sufficient scientific knowledge. In addition, the doctor-patient relationship changed; the modern physician no longer was concerned about the social and physical environment or about having a good bedside manner. Finally, the importance of the hospital increased, gradually leading to the elimination of the house call, which had characterized medical practice of the past.

Warner is to be congratulated for having produced a well-conceived intellectual history that substantially enhances our knowledge and understanding of nineteenth-century American medicine. His book is also a demonstration of the increasing sophistication of the field, as well as an example of what marvelous work can be done by a well-trained historian whose work is adequately supported by research grants.

MARTIN KAUFMAN
Westfield State College

BOBBY D. WEAVER. *Castro's Colony: Empresario Development in Texas, 1842–1865*. College Station: Texas A&M University Press. 1985. Pp. xiv, 158. $17.50.

Bobby D. Weaver relates the history of an empresario (land contractor) settlement during the republic and state periods of Texas history. With an easy writing style, the author briefly traces the overall background of Texas history in which the enterprise took place. The work then deals with the empresario, Henri Castro, in a very informative manner. The reader comes away with an appreciation for the problems facing all empresarios, and Castro in particular. Much of this fine work deals with Castro's efforts to settle West European immigrants on land granted by the Republic of Texas. Here the relationship between Texas land policy and the motives of an empresario become clear. Texas attempted to increase its population, and thereby its economic well-being, by following the old Spanish and Mexican policy of granting land to individuals who, in turn, were contractually obligated to bring in a specified number of settlers. The work relates how Castro, beset by a number of misfortunes, fulfilled a good portion of his contract with Texas. The study concludes with a description of life in the colony. The author provides an excellent analysis of the development of the community of Castroville, Texas, and the surrounding areas.

Like any work of this nature, Weaver's book would have been enhanced with some additions. A brief contrast and comparison of the empresario system during the Texas colonial period with that of the republic and state periods would have been revealing. The addition of a few more maps, properly placed in the text, would assist the reader in geographic perception. A list of colonists (placed as an appendix to the text) would have been helpful to genealogists. Also, the motives of the colonists in immigrating to Texas should have been better developed. And a broader background of Texas politics of the period would have given the reader a better understanding of Castro's colony. Although the author is too cautious in analyzing Castro's personality, the work is a fine piece of research and scholarship on regional and land history.

EDWARD LUKES
Hillsborough Community College

JAMES C. BRADFORD, editor. *Captains of the Old Steam Navy: Makers of the American Naval Tradition, 1840–1880*. Annapolis, Md.: Naval Institute Press. 1986. Pp. xvi, 356. $24.95.

With this second volume of his *Makers of the American Naval Tradition*, editor James C. Bradford merits congratulations for brilliantly continuing the start that he made in *Command under Sail, 1775–1850* (1985). In the present book he has deftly accomplished the personification of U.S. naval policies and tendencies through the careers of the officers who contributed to them. His authors are masters of their subjects, and their treatments are based on exemplary research in both primary and secondary materials, brought well up to date. Despite the inevitable stylistic differences in a multiauthored collection, Bradford has imposed on them a generally high literary standard that makes reading the book a pleasure. I have two suggestions for correction in later volumes: a subdivided index—the present one compels the user to grope among twenty-eight page references to "Welles, Gideon" and thirty to "Congress"; more maps are needed, and, incredibly, none is provided for the Wilkes Expedition, 1838–42.

Some authors can perforce contribute relatively little that is new about the more well known officers. This is true of the excellent articles by John C. Schroeder on Matthew C. Perry, Geoffrey S. Smith on Charles Wilkes, William N. Still, Jr., on David G. Farragut, and Tamara M. Melia on David D. Porter. Probably, most readers will learn more from the treatments of the individuals particularly oriented toward science, namely, David K. Allison's essay on John A. Dahlgren, William Stanton's on Matthew F. Maury, and Dean C. Allard's on Benjamin Isherwood.

Unfortunately, restrictions of space do not permit comment on all thirteen articles, but some deserve special emphasis. Charles M. Todorich's on Franklin

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Buchanan and Melia's on Porter combine to provide a first-rate capsule history of the U.S. Naval Academy's early years. K. Jack Bauer well summarizes the character flaws and contrary political trends that turned Samuel F. Du Pont from a significant peacetime naval contributor into an abject failure at Charleston in 1863. In Still's article Farragut emerges as the superb wartime commander that he was, but, disappointingly, only five short paragraphs are given to his career before 1861 (pp. 166–67), whereas the other authors devote an average of five to six pages to their subject's early career. Robert E. Johnson describes John Rodgers well but might have placed more emphasis on explaining the failure of the Rodgers-Low Korean Expedition of 1871. The two men had adopted a sensible plan to open negotiations with Seoul leading toward at least a shipwreck convention but embarked instead on a punitive campaign destined for miscarriage from the start. Frederick C. Drake's essay on Shufeldt is based on his monumental *The Empire of the Seas: A Biography of Robert Wilson Shufeldt, USN* (1984), and he admirably describes his cruise on the *Ticonderoga*, 1879–80. But he places insufficient emphasis on the American decision after 1865 to turn its back on the sea in favor of developing the West and industrializing. This, coupled with the rise of European imperialism, made impossible Shufeldt's dream of a revived navy and federally financed steamship lines to expand the nation's commerce in Africa and Asia. The reader will probably learn as much from Allard's contribution on Isherwood as from any other article, but why is he the only author who even mentions Charles H. Haswell—the first and leading engineer in the department, who served from 1836 to 1852—and that in a single short sentence? Elting E. Morison's "Inventing a Modern Navy" (*American Heritage* 37 [1986]: 81–96) persuasively argues Haswell's importance to the early steam navy. Probably, however, the article appeared while this book was in press, too late to be absorbed.

These few objections detract little from the conclusion that Bradford and his colleagues have made a significant and enduring addition to U.S. naval history, and we may anticipate the forthcoming volumes with relish.

DAVID F. LONG
University of New Hampshire

JACQUELINE BAKER BARNHART. *The Fair but Frail: Prostitution in San Francisco, 1849–1900*. (Nevada Studies in History and Political Science, number 23.) Reno: University of Nevada Press. 1986. Pp. xii, 136. $15.00.

In contrast to recent, convincing histories of American prostitution, Jacqueline Baker Barnhart's brief account of prostitution in San Francisco claims that many prostitutes were able to take advantage of economic opportunity and succeed in the ways of men celebrated by such historians as Daniel Boorstin. This was especially true in the earliest years of gold-rush settlement when enterprising prostitutes benefited from the sex ratio, high wages and profits, and the absence of settled values (including Victorian ones). Prostitutes, Barnhart insists, were either "professionals" self-disciplined enough to adapt to the law of supply and demand or else "not sufficiently professional to maintain self-discipline" and therefore failing, although she mentions the "dangers" of "venereal disease and unwanted pregnancy" (p. 38).

By the mid-1850s competition had sorted prostitutes into four categories. The bottom one included the Mexican prostitutes Barnhart adds to her summary of Lucie Cheng Hirata's history of enslaved Chinese women, but Barnhart concludes even this subject optimistically; she explains that other non-Anglo-Saxon-Americans, French prostitutes, were at the top because they were French and worked at being professional. Showing that some "working" women's prostitution resulted from "economic necessity," Barnhart sustains her theme by asserting that other such nonprofessionals entered the game out of the "general excitement" they democratically shared with San Francisco men and professional prostitutes (p. 64). Prostitution was "interwoven" with San Francisco culture because of the number and variety of people who profited from it. Prostitutes "worked in the most insecure part of the business" (p. 72), but Barnhart does not examine the distribution of their earnings, let alone other forms of power.

The book's argument is unsubstantiated; it neglects the comparative and other conceptual dimensions available in the works it cites; it neglects the body of relevant work on frontier and working-class women. It is frail but unfair.

G. J. BARKER-BENFIELD
State University of New York, Albany

EDWARD MAGDOL. *The Antislavery Rank and File: A Social Profile of the Abolitionists' Constituency*. (Contributions in American History, number 117.) New York: Greenwood. 1986. Pp. xvi, 172. $29.95.

By probing beyond the few who led to the many who were the mass of any movement we add breadth to vision and nuance to understanding. With similar aim the late Edward Magdol undertook this social profile of antislavery's rank and file. Based

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primarily on petition lists concerning major antislavery issues of the 1830s, the study draws additional prosopographical data from voter lists, census returns, city directories, church membership rolls, and tax and probate records.

The findings are rarely surprising, although they occasionally shed light on conventional understandings. Magdol's central hypothesis embraces an "intersection of the abolitionist movement with early industrialization in the Northeast" (p. xi), and he concludes, *inter alia*, that the antislavery rank and file in seven industrial cities were especially concentrated among skilled laborers but also among industrial managers and proprietors, that they were predominantly evangelical, that they owned minimal real and personal property, that in age they clustered between twenty and thirty, more broadly between twenty and fifty, and that most of them were born in Massachusetts (70 percent) or New England generally (89 percent).

These conclusions are less than satisfying—perhaps because the study was posthumously constructed from notes, incomplete drafts, and undigested raw data. Statistical data demand absolute clarity of definition, precision in explaining methods, and a clear focus on appropriate questions to be asked and results to be sought. Neither an explanatory appendix nor a minimally lucid text help. Furthermore, little attempt is made to use a comparative framework, to discover, at the very least, whether the characteristics of antislavery ranks reflected the wider population or deviated from it. More seriously, however, the reader feels an uncomfortable sense of partiality, for Magdol openly "wished to indulge a special interest—to make a case for factory operative antislavery activity" (p. 162). Therefore, although this book plausibly demonstrates a close connection between an industrial workforce and antislavery, one hesitates to accept the findings wholeheartedly.

Unquestionably, the complexities of the antislavery constituency deserve investigation. In the present case, however, the conclusions that emerge proceed more logically from the slim background of well-known antislavery historiography than from the principal sources of data. That is unfortunate, because the investigation itself is important, and the conception of the book, fundamentally sound.

WILLIAM H. PEASE
University of Maine

MALCOLM C. MCMILLAN. *The Disintegration of a Confederate State: Three Governors and Alabama's Home Front, 1861–1865*. Macon, Ga.: Mercer University Press. 1986. Pp. ix, 152. $16.95.

Malcolm C. McMillan uses the experience of Alabama to test Charles W. Ramsdell's thesis that the collapse of the home front was a major cause of the Confederacy's defeat. He concludes that Ramsdell was correct, at least regarding Alabama. He then traces the development of Alabama's virtually unsolvable wartime problems and describes how they sapped both the ability and the will of the people to fight and how the three governors handled these problems. The result is a well-researched and tightly written monograph centered on two major themes.

The first, and more familiar, theme describes the increasing number and intensity of Alabama's internal difficulties and the people's reactions to them. The problems of Governor Andrew B. Moore were relatively light and mainly involved indigent families of soldiers and disaffection in the north and southeast. But John G. Shorter and Thomas H. Watts found their state increasingly besieged by problems of defense, transportation, shortages of weaponry and manpower, arbitrary impressments, inflation, burdensome taxation, and civil disorder. Gradually, loyal Confederates became disillusioned with the war for independence, and the disaffected became disloyal and rebellious.

McMillan's chief contribution is his description of the responses of Alabama's governors to these crises. Moore and Shorter had been early secessionists, and as governors they were strong Confederate nationalists. Moore worked to improve the militia and the coastal defenses. Shorter impressed slaves for army labor, struggled unsuccessfully to maintain an effective militia, contracted for the manufacture of arms and uniforms, and instituted crop control and the regulation of alcohol distillation. Both governors vigorously engaged in price control, salt production, and poor relief. They had minor jurisdictional disputes with the Confederacy but in general cooperated well in the implementation of its policies. They could scarcely have done more, but Shorter found that his efforts increasingly alienated a despairing citizenry, and he was overwhelmingly defeated for reelection.

Much of Watts's support had been from those who longed for peace, but when he disavowed reconstructionism his popularity rapidly waned. His rigid states' rights principles placed him in opposition to most Confederate measures, and he quarreled with Richmond incessantly. But this won him little popularity with the masses, and even his constructive domestic measures were often blocked by a hostile legislature.

McMillan often comments that the absence of party politics and campaigning prevented the voters from evaluating candidates correctly. One might wonder whom Alabama might have chosen gover-

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nor in 1863 had Watts's opposition to reunion been generally known.

BUCK YEARNS
Wake Forest University

JOHN L. THOMAS, editor. *Abraham Lincoln and the American Political Tradition*. Amherst: University of Massachusetts Press. 1986. Pp. 162. $29.95.

The daunting task of shaping a collection of academic papers into a book has been performed ably by John L. Thomas. His introductory essay valiantly strives to provide these disparate contributions with a center of gravity that would make them worthy of his title. Yet the papers themselves, originally solicited for a conference at Brown University in 1984, resist his best efforts. All but one of them treat subjects only marginally related to the theme suggested by the title. Major differences in length, focus, and quality would give any editor reason to doubt the wisdom of this undertaking.

The collection opens with Robert H. Wiebe's excellent description of Lincoln's early social and cultural milieu. The only problem with this account is the author's assumption that Lincoln's great compassion as president can be attributed to his early socialization in the values of fraternal democracy. Wiebe's evidence plausibly could be used to argue that Lincoln's awkward attempts at camaraderie, including his misogynic style and smutty storytelling, reflected the anxieties of an outsider.

Three of the essays—the best, the worst, and the longest—deal with Lincoln as party leader. William E. Gienapp's quantitative study of the electoral coalition that put Lincoln in office, twice the length of other papers, concludes that new voters and former Know-Nothings provided the key margins of Republican support. Stephen B. Oates's offering, mercifully the shortest, is a passionate polemic denying the validity of David Donald's "Whig-in-the-White-House" thesis. To Lincoln scholars perhaps the most intellectually stimulating work is by Michael F. Holt. In a bold yet subtle presentation he argues that Lincoln "set out to destroy the Republican party as it existed in 1860," attempting to replace it with a new bisectional organization to be called the Union party (p. 117). Holt's perspective sheds new light on Lincoln's political priorities, including the troublesome issue of his attitudes toward Union and emancipation.

The only essay fully consistent with the book's title is by James M. McPherson. He addresses the broad theme of the Civil War as a second American revolution. But his conclusion, that Lincoln was a "conservative revolutionary," seems unsatisfactory. An oxymoron that simply reflects a major paradox of the tradition cannot serve as an analysis of it.

Also included is a contribution by Don E. Fehrenbacher on the scholarly uses and misuses of Lincoln's words. This piece does not fit with the others, but the author's careful readings and incisive observations enhance the value of the collection.

DWIGHT G. ANDERSON
San Diego State University

RICHARD H. ABBOTT. *The Republican Party and the South, 1855–1877: The First Southern Strategy*. (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1986. Pp. xiv, 303. $25.00.

A large body of excellent books and articles on the Reconstruction era has been accumulating for at least four decades. Significant new interpretations are now rare, and critically important discoveries in primary sources unlikely. Nevertheless, we still await the long promised synthesis. Any coherent study that helps organize the existing research into a credible explanation of a major component is therefore welcome. Richard H. Abbott has provided a lucid and persuasive focus on the perceptions of northern Republican leaders concerning opportunities for their party in the South after the Civil War. Despite some background from as early as 1855 and a one-chapter survey of the Grant years, Abbott's stated purpose is to illuminate Republican views and actions from 1865 through 1868.

Abbott's presentation suggests that Republicans in the North never expected permanent extension of their party into the South and were always primarily concerned with holding all northern states, if possible, in presidential elections. The establishment of voting rights for black men is traced more to vigorous activity by southern white Republicans and black leaders than to initiatives by leading Republicans in the North. Abbott offers voluminous evidence of the neglect of southern Republican interests in allocation of campaign funds and assignment of notable speakers. He calls attention to the repeated refusal of congressional Republicans to enact legislation that southern Republicans desperately needed to establish their credibility at home.

Acceptance of his thesis in full would require changing the usually accepted view of the intentions of most Republicans who enacted the congressional program in 1867. A major problem in demonstrating such a sweeping assessment lies in the absence of any official speakers for the party and the profusion of Republican leaders who may have exerted influence. Some of the evidence Abbott provides indicates high hopes for success in the South, and we cannot tell how many others instrumental in initiating the congressional program may have had optimistic expectations. A pervasive concern for carry-

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ing the states that had elected Abraham Lincoln in 1860 can be acknowledged without denying that many northern Republicans, unsure that this success could be repeated, looked southward for contingent support. Moreover, the ratification of the Fourteenth Amendment, surely a major objective of most northern Republicans, was not accomplished until the Military Reconstruction Act made possible ratification by several former Confederate states. Probably the greatest disagreement with the author will arise from his claim, not essential to the central argument of the book, that "Congress was primarily concerned with identifying, expanding, and protecting black rights" (p. xii). Anyone who has serious reservations about the book's major thesis, however, should marshal convincing evidence, for Abbott has based his effective presentation on thorough research.

THOMAS B. ALEXANDER
University of Missouri, Columbia

MERLINE PITRE. *Through Many Dangers, Toils, and Snares: The Black Leadership of Texas, 1868–1900.* Austin, Tex.: Eakin. 1985. Pp. xii, 260. $15.95.

Beginning with Thomas Holt's path-breaking 1977 study of black political leaders in Reconstruction South Carolina, historians have begun to take a closer look at local black politicians in the South. In this study of forty-one Texas legislators Merline Pitre seeks to examine "the nature and degree of these lawmakers' influence upon their constituents, as well as upon their white colleagues" (p. xi). Pitre also asks whether these black leaders shared any responsibility for the downfall of Reconstruction, formulated a clear and unified ideology, or represented the wishes of their constituents. To answer these queries, the author integrates the text with biographical vignettes and discussions of such topics as the constitutional convention in 1868, the various legislative sessions, the Republican party, and fusion politics. The concluding five chapters provide biographical sketches of Matthew Gains, a militant spokesman for equal rights, George T. Ruby, a Republican party loyalist, Richard Allen, an "opportunist," Robert L. Smith, an accommodationist, and Norris Wright Cuney, a complex and multidimensional political figure.

Despite a cumbersome writing style, including such phrases as "without a doubt" (p. 12), "it goes without saying" (p. 93), and "to be sure" (p. 102), the book contains some new information on several legislators, corrects the misinformation presented in J. Mason Brewer's 1935 study of black legislators in Texas, and discusses the wide range of views held by black leaders on such issues as racially mixed public schools, landlord-tenant relations, the employment of children, pensions for Confederate veterans, and civil rights. In general, black Reconstructionists, as the author suggests, "exhibited a surprisingly modern cast of mind" (p. 80). In other sections, especially those dealing with frontier violence and lawlessness, the study makes a contribution to the literature. The book would have greatly profited from the use of roll-call analysis techniques, a more detailed comparison of black legislators in Texas with those in other southern states, and an in-depth analysis of various groups of sympathetic whites. Without these the study has a static quality, lacking a full appreciation for the interracial dynamics so important to understanding Reconstruction and post-Reconstruction southern history.

LOREN SCHWENINGER
University of North Carolina, Greensboro

MARK E. NEELY, JR., and R. GERALD MCMURTRY. *The Insanity File: The Case of Mary Todd Lincoln.* Carbondale: Southern Illinois University Press. 1986. Pp. xiv, 203. $19.95.

In 1975 Robert Todd Lincoln Beckwith, the last direct descendant of Abraham Lincoln and Mary Todd Lincoln, and James Hickey, the curator of the Lincoln collection at the Illinois State Historical Society, discovered documents that related to Mary Todd Lincoln's insanity trial in Robert Todd Lincoln's mansion. Eventually, Beckwith turned the file over to two well-known Lincoln specialists—Mark E. Neely, Jr., and R. Gerald McMurtry.

The result is the present book, which, along with a complete description and analysis of Mary Lincoln's trial, confinement, and retrial, includes forty pages of previously unpublished letters of Mary Lincoln and twenty-four pages of photographs of the principals in this affair. Neely and McMurtry cover events that took place within a period of thirteen months, beginning with Mary Lincoln's first trial in 1875 and ending with her battle to control her money. In June 1876 another jury found her restored to reason and competent, in judicial language, to have charge of her property.

Neely and McMurtry eschew any desire to take sides in this controversial episode. But their disclaimer that they "experienced fascination but not moral engagement" is disingenuous, since the book is based on documents exculpatory to Robert Todd Lincoln, their collector (p. xii). For example, to prove he had taken nothing from his mother, Robert kept only those fragments of her letters in which she had earlier given him possessions that she demanded he return after his testimony in her trial. At the end of the volume all the principals are hauled

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before Neely and McMurtry's bar of justice for a final historical judgment.

Still, this is a fair-minded, comprehensive account based on new documents. Unlike their predecessors, Neely and McMurtry have placed Mary Lincoln's trial within the legal, social, and medical context of the times. So viewed, what happened to Mary Lincoln was governed as much by procedures and collective attitudes as by personal motivations. Although the book tells the tragic story of a special American family, it transcends Lincolnalia and can be read for its professional treatment of several aspects of late nineteenth-century social history.

Tolstoy once remarked that "all happy families resemble one another; every unhappy family is unhappy in its own fashion." Readers of this book will wish Abraham Lincoln's wife and only surviving son had picked some other way to express their unhappiness.

JEAN BAKER
Goucher College

JAMES A. YOUNG and B. ABBOTT SPARKS. *Cattle in the Cold Desert.* Logan: Utah State University Press. 1985. Pp. xxi, 255. $27.50.

The time was not so long ago or far away when histories of ranches read like military exercise manuals. This school of historians followed the dictum of "have ranch records, will write history." As a result, duplication piled on duplication. If you had read one, in effect, you had read them all. Not so in recent years, for a metamorphosis has occurred in subjects treated and in narratives produced. Lawrence M. Woods recently published *It Was Hard to Be a Gentleman* (1986), the tale of Moreton Frewen's Wyoming Powder River ranch; it is one of the most sophisticated existing analyses of economic investment of the cattle industry of the 1880s.

The present book represents another line of departure. The regional range ecology of the late nineteenth century is described through the operations of three cattle barons: John Sparks, A. J. Harrell, and John Tinin. Of course, many shelves of studies have been devoted to range ecology, from W. C. Barnes to R. R. Robertson to Ronald Scobey. James A. Young and B. Abbott Sparks have, however, related the economic rise and fall of John Sparks and his associates through the land, until the future of land and man converge.

The life and death of John Sparks has chapter headings typical of many a cattleman. Trained by his neighbors, the remarkable Snyder brothers of Georgetown, Texas, John Sparks first pioneered the Chugwater Valley of Wyoming. Exhausting the range land in Wyoming, in 1883 John Sparks moved to northern Nevada and southern Idaho. In his baggage he brought the "talents" of credit, managerial know-how, and, above all, the confidence of a successful entrepreneur. Eighteen years later, he was bankrupt. Why? John Clay, that brilliant livestock financier and astounding gossip, believed that the reasons for his demise were manifold: low cattle prices, winter losses, poor mining investments, and his life style in general.

Young and Sparks do not disagree with Clay's diagnosis. But they note that John Sparks, in common with many of his contemporaries, ignored the doomsday of environmental compatability. Wintering cattle in the Salt Deserts, where shrubs, not grasses, came to dominate, was an enormously exploitive practice. The depletion of range became catastrophic with the hard winter of 1889–90, which almost destroyed the cattle country of the intermountain region. One lone statistic tells all for John Sparks—in 1885, he branded 35,000 calves, in 1890 only 68 cows. Irrigation and hay meadows became the partial answer, as he converted from the labor-extensive range economy to a labor-intensive irrigation enterprise. The transformation was too late for the sagebrush-grasslands. The forage of his winter range had been severely depleted. The grasses had been exhausted, leaving the shrubs to the range sheep industry.

What of the range of the twenty-first century? Young and Sparks are not optimistic. Unless a cultural transformation occurs, the perennial sagebrush-grasslands will probably be reduced to annual grasses and weed ranges. The biblical text of Young and Sparks is clear—will it be ignored?

GENE M. GRESSLEY
University of Wyoming

ROBERT W. LARSON. *Populism in the Mountain West.* Albuquerque: University of New Mexico Press. 1986. Pp. x, 210. $27.50.

This volume represents an effort to interpret, in a broad and generic sense, "Populism" in the mountain and intermontane regions of the United States. Robert W. Larson's primary focus is on the "front range" states and territories (Montana, Wyoming, Colorado, and New Mexico), but he also devotes some attention to Idaho, Utah, Nevada, and Arizona. Although third-party activity dominates the study, Larson also considers the economies and the major party politics of the relevant political entities.

Larson's fundamental purpose is to destroy the presumably pervasive notion that Populists in the mountain states concerned themselves exclusively with the free coinage of silver. Such notable scholars as John D. Hicks, Richard Hofstadter, and Lawrence Goodwyn dismissed Populism in the region as mere "silverism." On the other hand, the ample

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monographic, periodical, and thesis literature that Larson synthesizes and that forms the evidentiary basis for his book forces one to conclude that, before the Populist-Democratic fusion of 1896, silver was not a major Populist issue in the region. In a sense Larson has arbitrarily created, and proceeded to destroy, a straw man.

Nevertheless, Larson clearly illustrates that Populism in the region was a multifaceted movement tied together by a common thread of antimonopolism. The monopolies that provoked third-party activity varied with individual states and territories but tended to be railroads, land and cattle companies, water companies, or mining corporations. Indeed, in some states (especially Colorado and Montana) Populism largely reflected the demands of organized labor—the eight-hour day, mine safety, the prohibition of child labor, and the exclusion of the Chinese. At their party convention in 1894, moreover, New Mexico's Populists did not even mention the silver issue, whereas in some states and territories Populists advocated silver coinage as a means of restoring full employment in the depressed mining industry rather than as an inflationist device.

In this matter-of-fact monograph Larson establishes a number of salient points about Populism. He demonstrates that the standard agrarian issues of the movement were of secondary importance in the Mountain West, and he shows that the major parties in the region were often more interested than the Populists in reestablishing the legal coinage of silver. But the reader must grasp the most significant contribution of the book solely by implication. After more than two generations of scholarly debate about Populism, a satisfactory conceptualization of the phenomenon, one that accounts for its subspecies and regional idiosyncracies, is still lacking.

KAREL D. BICHA
Marquette University

JOAN M. JENSEN and SUE DAVIDSON, editors. *A Needle, a Bobbin, a Strike: Women Needleworkers in America.* (Women in the Political Economy.) Philadelphia: Temple University Press. 1984. Pp. xxii, 304. $29.95.

Joan M. Jensen and Sue Davidson have brought together a strong collection of articles on women and the garment industry in America since the mid-nineteenth century. The articles grapple with the structuring of women's work by the economic system and cultural values and with the relationship between women's work, consciousness, and liberation. Feminist and labor historians have done much research on the textile industry and domestic service, but sewing, the other major occupation of American women in the nineteenth century, has received less attention. This book begins to redress that imbalance and suggests that more research on this topic would reveal much about the dynamic between the exploitation of women by the market and their subservience in the family.

The collection contains nine articles grouped in three sections, each introduced by Jensen. In the first section, on the industry in the nineteenth century, Jensen's introduction describes the nature of needlework as a domestic, nonmarket activity and of the sextyping of different kinds of sewing work prior to mechanization. The first article, by Ava Baron and Susan E. Klepp, is an excellent example of historical sociology. It explores the timing and extent of mechanization in sewing and develops a sophisticated analysis of causation that includes economics, technology, demography, and cultural values. Deborah S. Gardner's article on A. T. Stewart's department store examines the implications of the new sewing technology for women for different classes and for the relationship between them. These articles emphasize the extreme vulnerability of women during the Industrial Revolution because of the sextyping of jobs. The authors do not examine, however, the efforts of seamstresses to organize or to control their work.

This issue is the focus of the second section, on the "great uprising"—the strikes and organizing campaigns between 1900 and 1920. Following Jensen's survey of organizational activity are articles on four cities—Chicago, New York, Rochester, and Cleveland—which reveal significant differences in the outcome of the strikes and in the extent to which middle- and upper-class women aided working-class women. Many concerns should be addressed in approaching this topic: ethnic divisions in the labor force, the economic structure of the industry and of each city, the relationship between male and female workers, and the roles of the Women's Trade Union League and wealthier women. Each author has a different emphasis. Jensen's article on Rochester and Lois Scharf's on Cleveland focus on gender issues, whereas Sue Weiler's work on Chicago and Ann Schofield's on New York are stronger on the ethnic complexity of the movement, although neither ignores feminist concerns. Weiler uses interviews with strikers and examines the personal meaning of the strike for them. She recounts the elation that the young women felt on taking action but also the limited role the strike had in forming their consciousness or in changing gender roles.

In her piece on New York Schofield reflects on the historiography of the "great uprising" and examines the role of Jewish socialism in creating the union movement. Schofield comes closer than the others to answering the question that should be asked but is not: why did organization take place between 1900 and 1920 rather than before or after-

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ward? But Schofield's focus on New York precludes general statements about the movement. Scharf is the only one of the authors to examine the nonstrikers in detail, and this is necessary to a fuller understanding of the union movement. Although these articles are interesting, this is the least satisfying section, because it lacks an interpretive conclusion. The role of various ethnic groups and their cultures in the union movement remains unclear. A conclusion might also suggest why women allied across class lines in the two largest cities but not in the smaller ones. How and why men dominated the unions in this industry with a female majority also demands analysis.

A major theme of the third section, on the industry since 1920, is how the unions became anachronisms as the ethnic composition of the labor force changed. The industry has remained labor-intensive, small scale, and extremely competitive. This has led employers on a constant search for new groups of easily exploitable women, and they have found new workers among illegal aliens and welfare mothers in big cities, Chicanas in the Southwest, and the poor in the Third World. The white male union leaders have retained control but lost touch with the workers. Elizabeth Weiner and Hardy Green show that this has led to a revival of the worst sweatshop abuses in the heart of union territory, New York City. Nina Asher's biographical piece on organizer Dorothy Jacobs Bellanca explains much of the unions' declining appeal to workers, for Bellanca had to work within the narrow limits laid down by the men and to see her more innovative plans for organizing women put aside.

The single most important strike of garment workers since the 1930s was that of the Chicana workers against the Farah Company in the 1970s. In an excellent article, Laurie Coyle, Gail Hershatter, and Emily Honig use oral history to show the meaning of work and organizing to these women. Their interviews reveal the personal growth the women experienced through challenging authority. This is a central theme of modern feminism, and in this respect a marked difference separates the Chicana workers of the 1970s, who moved away from the traditional subservience of mothers, from the Jewish and Italian strikers of 1910. In other respects the story of Farah is a study in continuity, the power of industry to evade workers' control, the difficulty women have gaining power within unions, and the interdependence of women's family and working lives.

SUSAN E. HIRSCH
Northwestern University

TIMOTHY J. MEAGHER, editor. *From Paddy to Studs: Irish-American Communities in the Turn-of-the-Century Era, 1880 to 1920.* (Contributions in Ethnic Studies, number 13.) New York: Greenwood. 1986. Pp. xiv, 202. $35.00.

This collection of well-written and perceptive essays examines six Irish-American urban communities from 1880 to 1920. The communities covered are those of Lowell, Worcester, Philadelphia, Chicago, St. Louis, and San Francisco. Also included in the book is an insightful essay by Kevin O'Neill on the changes that occurred in post-Famine Ireland.

The years from 1880 to 1920 saw significant changes among Irish-Americans. During this period, as ever increasing numbers of American-born Irish reached adulthood, Irish-American urban communities ceased to be overwhelmingly populated by immigrants. Although Irish economic and social mobility was relatively slow in some cities, greater numbers of Irish-Americans entered the middle class. Irish political strength also increased. The cities in which the Irish lived grew larger and more complex, and their populations became more heterogeneous with the influx of the "new immigrants."

The essays note Irish-American involvement in the Irish nationalist struggle; Dennis Clark's study on Philadelphia and Timothy Sarbaugh's on San Francisco provide the most extensive coverage. Although the cause of Irish freedom achieved wide support among Irish-Americans at certain times, the essays as a whole uphold the general consensus that the most important Irish-American institution was the Catholic church. The essays of Brian Mitchell on Lowell and Ellen Skerrett on Chicago give particularly interesting accounts of the activities of local priests.

All the essays support the view that Irish attachment to Catholicism remained strong throughout the period, but certain authors indicate that Irish identity waned considerably. Although noting that the Worcester Irish strongly supported the Irish nationalist struggle during the Anglo-Irish War (1919–21), Timothy J. Meagher contends that by 1920 the Irish in that city were more Catholic than Irish. Similarly, Martin Towey asserts that after the turn of the century the second- and third-generation St. Louis Irish "were identifying themselves more as Catholic Americans than as Irish Americans" (p. 155), and Skerrett claims by 1920 in Chicago "to a large extent Irish identity had been transformed into a strictly Catholic identity" (p. 119). Skerrett points out that, by ignoring the Irish heritage of their students, Catholic schools did much to blunt the sense of Irish identity among the American-born Irish.

These essays call attention to the diversity of Irish-Americans. Time of entry, the economic condition of the first wave of Irish immigrants, and the

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population mix of a city as well as other factors shaped the various Irish communities in distinctive ways. For example, the Irish in both San Francisco and Chicago had the advantage of arriving in new cities without entrenched Anglo-American elites. However, since San Francisco attracted more prosperous Irish settlers, many of whom had previously lived in other American cities, whereas the Chicago Irish community was quickly inundated with impoverished immigrants, the San Francisco Irish established a significant middle class long before the Chicago Irish did. Noneconomic factors were also important. Because St. Louis was a multiethnic city with an old, if small, respectable Catholic community, the Irish there never experienced the more virulent forms of nativism encountered by the New England Irish.

Although some of the essays fail to give adequate coverage to certain dimensions of Irish-American life, all of them have much to offer the specialist as well as the general reader. This book is a valuable addition to the growing body of literature on the Irish in the United States.

MICHAEL F. FUNCHION
South Dakota State University

MARTIN E. MARTY. *Modern American Religion.* Volume 1, *The Irony of It All, 1893–1919.* Chicago: University of Chicago Press. 1986. Pp. xi, 386. $24.95.

This book is the first of a four-volume examination of American religion from the end of the nineteenth century to the end of the twentieth. The book is ambitious because Martin E. Marty is eager to escape formal, that is denominational, history. He covers, certainly, central religious bodies but also considers groups that were neither numerically significant nor significant for upper-middle-class Protestant life. More important, Marty treats the dimensions of religion that were connected to gender, race, and ethnicity independently of creed, and he considers the relation of religion to politics, foreign policy, the arts, and general literary endeavor. More than the usual "history of religion in America," the book is a history of a religious culture. These notions are not new, nor are they new to Marty, but they find their most elaborate expression in this book.

The book has five parts. The first two are devoted to the self-conscious religious "modernists"—the late-nineteenth-century Protestant liberals—and the "moderns"—the academic professionals in philosophy and the social sciences who ostensibly rejected religion. Some quibbles about Marty's grasp of these elites may arise, but in general he effectively reads these figures, summarizes the secondary literature, and interprets the result.

The next two parts, on "modernity" and "countermodernity," deal with the response of varied American religious groups to the challenge of the twentieth century. These two sections are most successful in synthesizing a vast amount of material around a broad notion of adaptation. At the same time Marty never makes clear why or how the people of parts 3 and 4 reacted to the tiny number of intellectuals discussed in parts 1 and 2. Yet this is the impression the structure of the book gives, and no other explanation is offered of the adaptation of the peoples.

The last part, on "transmodernism," is least satisfactory. Here Marty discusses a diverse collection of those who went beyond the accommodations of religion in the late nineteenth century to a new attempt at harmonizing human beings and the cosmos. Marty's collection is itself not harmonious—thinkers such as Josiah Royce and George Herbert Mead, "mind cure" theorists such as Ralph Waldo Trine, the Federal Council of Churches, proponents of the Social Gospel, and Mormons, among others.

The title of the book names the chief explicit interpretive device: irony. The progressive Protestants came to be seen as naive in their facile adjustments; the conservative "countermoderns" who sought to return to simple forms of faith spawned new sects and enunciated novel doctrine.

The book is not easy reading. Marty has looked at many literary, theological, philosophical, and social scientific theorists and uses their insights in his first chapter to conceptualize his study. But, whatever critics make of Marty's abilities in this sphere, his attempt is fundamentally half-hearted—hat doffing to Reinhold Niebuhr, Clifford Geertz, Ernest Gellner, and the like. The book is really carried along by his sense of irony and by a series of competing and consciously used metaphors. Religion functioned at various times like a carapace or a canopy or operated through a cocoon. Because these extended images are vague and sometimes inconsistent, they confuse more than they clarify. They are, however, of a piece with Marty's tendency to write in a style that, though not oracular or precious, fails in its attempt at eloquence. Nonetheless, the book is more than a "contribution" to the literature. The author has made a serious effort to recast our understanding of American religious life, and that deserves a modicum of praise.

BRUCE KUKLICK
University of Pennsylvania

JAKE W. SPIDLE, JR. *Doctors of Medicine in New Mexico: A History of Health and Medical Practice, 1886–1986.* Albuquerque: University of New Mexico Press. 1986. Pp. xvi, 384. $29.95.

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Jake W. Spidle, Jr.'s, book is the first substantial history of health, disease, and medical activity in New Mexico. Although making no claim to comprehensiveness—it includes little or nothing about Indian medicine, the Spanish heritage, or alternative medicine—it provides a thorough, if overly doctor-centered, account of mainstream medicine in the state. It is interesting, informative, well researched, but sometimes annoying.

This publication resulted from a praiseworthy joint undertaking by the author (an academic historian), the library staff of the University of New Mexico's medical school, and the state medical society to identify, collect, preserve, and put to use materials relating to the medical history of the state. One immediate aim of the book was to commemorate the state medical society's centennial, but it does much more than that. Three chapters survey the creation and growth of medical societies, hospitals, a medical school, and other orthodox medical institutions from the territorial period to the present. Two broader chapters deal with the early flourishing of New Mexico, following the coming of the railroad, as a mecca for sufferers from tuberculosis—a circumstance that stimulated an enormous increase in population, made the building of sanatoriums into big business, and raised the quality of medical practice. A chapter of equal significance depicts the historical contributions of female practitioners in reducing maternal and child mortality, particularly in rural areas, and another examines the state's substantial sanitary problems and the slowly evolving public health responses.

A wealth of maps, charts, and tables covering distribution of physicians, out-of-state medical schools attended by early New Mexico doctors, disease and mortality rates, and other topics adds welcome depth to the narrative account. Of less merit, except perhaps for antiquarians, is the large number of profiles of New Mexico physicians, many of them accompanied by photographs. Since relatively few of the doctors portrayed—however "hard-working," "dedicated," and "respected" they may have been—are shown to have had any special distinction, the reader would have been better served by greater selectivity.

The author has nevertheless produced a sensitive and worthwhile work. It would have been a better one if editorial attention had been given to an excessive number of irritating stylistic conceits and lapses. The content of the book could also have been improved if Spidle had stood further back intellectually from his subject and, particularly, from the essentially whiggish view that members of the medical profession tend to have of their past.

JAMES H. CASSEDY
National Library of Medicine
Bethesda, Maryland

RICHARD RUDOLPH and SCOTT RIDLEY. *Power Struggle: The Hundred-Year War over Electricity.* New York: Harper and Row. 1986. Pp. xiii, 305. $19.95.

The social history of public electric power (municipally owned and cooperatives) is an important part of the history of technological development in the United States. The country is at a critical juncture in the evolution of the employment of electric energy for industrial and household use. Richard Rudolph and Scott Ridley's premise is that the history of decision making about the generation and distribution of electric power needs to be studied before decisions about the future can be made.

Will that future be a continuation of the large central power station, deriving energy from nuclear fission or from coal, or will the future be one of conservation, decentralization, and alternative sources of energy, such as the sun? The authors state the issue about the energy future in the United States in stark terms: "More than a debate about technology, it is a fundamental issue of democracy" (p. 19).

The democracy referred to is cited as that which ought to be used in making decisions about energy. The groundswell of public reaction to higher rates imposed to fund nuclear plants has spread across the country, and, the authors warn, the past decade of fighting higher rates "may only be the start of a larger clash on the horizon" (p. 237). The public, once aroused by the threat of higher rates, has gone on to push for public ownership in New Orleans, San Francisco, Chicago, Long Island, and smaller communities across the country.

The authors' main argument is that democratic decision making will result in choosing the best technology. The definition of "best," one supposes, is that technology that is most compatible with our culture. One possibility for an energy future is that of decentralized sources of energy producing electricity with benign, renewable resources. Predictions, such as that one, are based on little experience. For example, we have already learned that wood-burning stoves are not benign to the air we breathe.

The subtitle of this book refers to the struggle over public versus private ownership of the country's electric power systems. That "war" as depicted in the book has been concentrated in the last thirty-five years or since the advent of nuclear energy. Rudolph and Ridley only briefly describe earlier conflicts.

The story of that "war" as told in this book does not make a convincing case for a future of publicly owned electric power systems with grass-roots decision making. An argument has been made by such strong advocates of public power as Leland Olds for a mixture of public and private power companies.

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What that mixture might be is not addressed in this book, which the authors chose to make a polemic rather than a history.

HAROLD ISSADORE SHARLIN
HIS Associates
Washington, D.C.

DENNIS C. DICKERSON. *Out of the Crucible: Black Steelworkers in Western Pennsylvania, 1875–1980.* (SUNY Series in Afro-American Studies.) Albany: State University of New York Press. 1986. Pp. xiv, 323.

Most scholarship relating to urban blacks has concentrated on the process of ghettoization. Obviously, this is an important line of inquiry, but studies that examine black labor within the framework of ghettoization do not adequately reveal the dynamics of racial discrimination within the process of production. Dennis C. Dickerson breaks with tradition and argues that "the historian must focus upon the industry rather than the ghetto community as the principal determinant of the Black worker's economic status" (p. 5). Following his own dictum, the author examines the century-long struggle to achieve equality of opportunity in the greater Pittsburgh steel industry.

Although considerable attention is focused on the relations between black steelworkers, the white unions, and the employers, this is a working-class history that also examines the black communities within the western Pennsylvania steel cities. Dickerson analyzes the role of black strikebreaking in the late nineteenth and early twentieth centuries and the basis of class fragmentation, social strains resulting from the migration of southern blacks into the three rivers area during World War I, and the failure of industry efforts at welfare capitalism between World War I and the Great Depression. Blacks were not assimilated into white social institutions, so they created their own parallel structures, and the author assesses the roles played by community organizations such as churches, lodges, the Urban League, and the National Association for the Advancement of Colored People.

Internal black organizations figure prominently in this study, but the most significant external institutions affecting the lives of black steelworkers were the companies and the unions. Discriminatory companies limited black employment to the hardest, dirtiest, most dangerous, and lowest-paying jobs, and the unions did not protect blacks from discrimination in hiring, firing, promotion, and job assignments. The Amalgamated Association of Iron, Steel and Tin Workers followed custom by systematically barring blacks from membership during the early years, and, even though blacks played an important role in the struggle for recognition of the United Steelworkers of America, their grievances were not redressed. The federal government first intervened to rectify this situation during the 1940s through the Fair Employment Practices Commission, but not until passage of the Civil Rights Act of 1964 was any measurable progress made toward equal opportunity for black steelworkers. Under pressure from the federal government, a plan for elevating the occupational status of blacks in the industry finally was welded together in 1974 between the union and several major steel corporations. Ironically, just when the 1974 consent decree forced the issue of parity in the employment of blacks and whites to the forefront, the steel industry entered a period of decline that resulted in wiping out not only the progress achieved under this plan but also the jobs of many of the workers as well.

Thoroughly researched in primary sources, this book is a sound piece of scholarship on an important topic and a useful addition to the growing body of literature on black labor in America.

RONALD L. LEWIS
West Virginia University

GARY ROSS MORMINO. *Immigrants on the Hill: Italian-Americans in St. Louis, 1882–1982.* Urbana: University of Illinois Press. 1986. Pp. xi, 289. $29.95.

Gary Ross Mormino's readable account of St. Louis's well-known Italian-American enclave "the Hill" focuses on why and how this settlement—unlike many immigrant neighborhoods—survived and flourished to become "an ethnic Williamsburg" by the 1960s. Not a typical downtown settlement, the Hill developed as migrant Italians found work in nearby clay mines and tile works. Lacking transportation links to other parts of the city, it attracted a relatively homogeneous population of Lombards and, later, Sicilians. Its empty lots and single-family housing allowed residents to remain in their neighborhood while purchasing a home—an important goal for blue-collar Italians. Although he gives due attention to these characteristics, Mormino finds the development of community institutions of greater importance in ensuring the Hill's survival. Here, readers might disagree with him: formal institutions can as plausibly be considered the outgrowth of community solidarity as its source, especially since most of the institutions Mormino so well describes developed when other immigrant neighborhoods began to decline.

Mormino's chapters on community change and institution building in the years after 1920 are the best in the book, and they constitute a particularly valuable contribution since most studies of immigrant communities end in 1920 or 1930. Scholars and students alike will learn much from Mormino's

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portrayal of bootlegging as a cottage industry, his description of the emergence of formerly apathetic Hill aliens as an ethnic voting bloc, his welcome attention to the development of immigrant sports associations and to the growth of an Italian-American parish, and his analysis of the crisis of World War II. This is ground-breaking work in our understanding of Italian-American communities, usually best known for their institutional weaknesses.

In his introduction Mormino emphasizes his dependence on oral accounts. His use of one hundred interviews makes his telling of Hill history a wonderfully human one. For this reason, Mormino's casual discussion of his sources is particularly frustrating. He does not, for example, describe the format of his interviews. Footnotes reveal that Mormino drew heavily on a relatively small number of oral histories. His best informant was a former mailman; the others he cites frequently were activists in parish and political clubs or local businessmen. Female voices appear infrequently; the author depended most heavily on women's accounts in his chapter on European origins and the early days of the community—before institution building. Did women's memories not fit easily into an account that makes formal institutions central to community survival? Also missing is any sense of real conflict on the Hill. Did one hundred interviews reveal no gossip, disagreements, or local variations in the community's understanding of its past? An appendix that systematically summarized and evaluated the interview material would have made this book more useful to scholars hoping to learn from Mormino's considerable expertise.

DONNA GABACCIA
Mercy College

ROBERT G. ATHEARN. *The Mythic West in Twentieth-Century America.* Foreword by ELLIOTT WEST. Lawrence: University Press of Kansas. 1986. Pp. xi, 319. $25.00.

When John Wayne died, the U.S. Congress quickly ordered a new medal struck—the "John Wayne, American, Medal." This should have alerted the public, and even the intellectuals, that not the West but what we thought was the West had genuine mythic staying power. In the distinguished historian Robert G. Athearn's last book, readers will find many such anecdotal gems. Athearn, like Charley Russell, was a master storyteller. The skill must derive from being born somewhere in Montana. But then, the West must have always inspired storytellers—mouths to match its mountains. And, because so many stories derived from both bona fide westerners and dudes such as Theodore Roosevelt and Frederic Remington, the tales and images of the pioneer West blended together to constitute our unique national myth. As the late Denis Brogan put it, "The United States is the only country since the Middle Ages that has created a legend to set beside the story of Achilles, Robin Hood, Roland and Arthur." Myth, legend, epic, and storytelling reverberate through Athearn's sometimes nostalgic and even autobiographical look at the twentieth-century West, exclusive of California and Texas—entities that seemingly defy classification.

The book, however, includes or tries to do too much. Like a plum pudding, it is full of good things, but it has no shape. Athearn at first structures his chapters chronologically, that is, according to the eras of the cowboy, the farmers, the onset of towns, the Great Depression, and so on. Later, he switches to topics in chapters such as "Colonialism: The Enduring Dilemma," "The Fictional West," and "The Wilderness Evangelists." Along the way, however, Athearn does strike off such phrases as "the wilding up of the West" and notes in passing that there had been at last count 156 genuine Deadwood Stagecoaches. He also adduces some striking statistics: for example, in 1983 some 327 million people, or one and one-half the population of the United States, visited the western national parks. He also unearths hard-to-get statistics on publishing and motion picture attendance. The style and tone of the book are informed but informal, appealing, yet filled with clichés and snappy quips: attending his classes must have been fun.

Most valuably, the book demonstrates just how abundantly the myth of the Old West thrives in the twentieth century, both in the United States and abroad. Athearn is hard put to account for the staying power of that myth. With latter-day changes and different attitudes visible all around him, he can easily debunk the myth himself. He perhaps fails to realize that, despite economic and social changes, the myth of the Old West is as much a part of western reality as its mountains, rivers, tract-house bedroom communities, perpetual passive-aggressive colonial stance, and (with a nod to T. S. Eliot) "asphalt roads and a thousand lost golf balls." In trying to explain the myth of the Old West, Athearn looks in the wrong place. He amusingly exhausts the possibilities of the West as phenomenon, but for him the noumenal West remains the unexplored terrain of the mind.

WILLIAM H. GOETZMANN
University of Texas

MARK COWETT. *Birmingham's Rabbi: Morris Newfield and Alabama, 1895–1940.* (Judaic Studies Series.) University: University of Alabama Press. 1986. Pp. xiii, 222. $22.95.

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With the activation of the Southern Jewish Historical Society and the proliferation of books on southern Jewish history, this field has come into its own. The present biography of Morris Newfield, spiritual leader of the Reform Temple Emanu-el of Birmingham from 1895 to 1940, and Mark H. Elovitz's *A Century of Jewish Life in Dixie: The Birmingham Experience*, published over a decade earlier, have captured the essence of this small southern Jewish community. Both volumes describe the sociological and ideological tightrope traversed by an ethnic leader such as Newfield whose success depended on his acceptability by the Christian community in consonance with his defense and articulation of Jewish values. But Newfield's posture as an American rabbi in the South is typical of many other colleagues and is an oft-told tale. Does his career then justify full-length biographical treatment?

According to Mark Cowett, previous efforts in American Jewish history have been parochial; they have disregarded relationships with other ethnic groups and with society at large. Cowett, therefore, as his major theme, labels Newfield an ethnic broker, which relates not merely to the conservative spirit of Birmingham's Christian leadership but also to his involvement with liberal or progressive Christian efforts in this southern community. The identification of Newfield with liberal Protestant ministers promoting their version of the Social Gospel seems at variance with the usual portrayal of religious leadership in the South. This observation certainly merits serious consideration.

This approach, however, has also created a major shortcoming. The treatment of Newfield's biography, ideology, leadership of Birmingham Jewry, and stature within the general community in separate but unequal chapters covering overlapping chronological periods makes for an unbalanced, repetitive literary product. Particularly annoying are the author's interjections of what had been previously "traced" and what he intends to "explore"—expressions manifest more in doctoral dissertations than in published works.

The nonspecialist will be interested in Newfield's attempt to negate the influence of the Ku Klux Klan, which plagued Birmingham in the 1920s. In keeping with the caution he displayed in the early years of his career, Newfield sought to confront social issues through indirect means such as interfaith forums rather than through direct and open attack. His quiescence in the case of the Scottsboro Boys, incarcerated in Birmingham, was an indication of a fear that involvement in racial issues would jeopardize the Jewish community and his own position of leadership. Subsequently, Newfield became more assertive of Jewish ethnic differentiation in abandoning non-Zionism for an espousal of a Jewish homeland in Palestine.

Newfield's career in Birmingham parallels that of other pivotal colleagues in the South. He was an advocate of Reform theology, a Social Darwinist, a spokesman for the Jewish community, and a patron of various generic and Jewish philanthropies. Cowett successfully describes Newfield's devotion to the tenets of classical Reform theology in tandem with his emphasis on Jewish religious specificity. Cowett's study of the rabbi of Birmingham has literary deficiencies, but segments of the biography deserve more than passing interest.

MYRON BERMAN
Richmond, Virginia

DON S. KIRSCHNER. *The Paradox of Professionalism: Reform and Public Service in Urban America, 1900–1940*. (Contributions in American History, number 119.) New York: Greenwood. 1986. Pp. xiii, 195. $32.95.

Don S. Kirschner has taken on a significant subject: the professionalization of reform elites in social welfare, public health, and city planning over the first four decades of the twentieth century. By so defining the central theme of his work he provides two valuable services: his analysis traces the interconnections among three overlapping professions that addressed urban concerns from different perspectives; and, whereas so many other studies have focused on either the Progressive era or the New Deal, his account spins out their internal dynamics as they developed from an early sense of moral crisis in American life, into an era of "normalcy" when the political mood was not generally hospitable to reform issues, through the 1930s when the influence of experts on the shaping of social policy and its implementation crested.

Kirschner is sensitive to the tensions and ambiguities that characterized the process of professionalization: the commitment to individual rights and welfare on the one hand, the need for collective action on the other; the early hope that moral regeneration would follow social reform, and the growing emphasis on objective and value-free programs; the dedication to cultural pluralism, and the gathering force of nationalism. He also highlights the contest between forces intrinsic to professionalization that focused on elements internal to the processes of specialization and extrinsic elements operating in society generally. As for "social control," a concern on which so much historical analysis of reform has centered, Kirschner concludes that leaders in social work, public health, and urban planning "saw no conflict between welfare and control" but saw them as "complementary"—"the repression of certain liberties by imposing controls in the name of public welfare" (p. 180). Fit-

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tingly, the book concludes with questions bearing on a persisting dilemma of professional reform: "How was it possible to create a feeling of community in industrial society without vast subsidies from the central government? Yet how was it possible to have centralized subsidies without the centralized controls that would almost certainly destroy any last vestige of community feeling" (p. 187)?

In his research the author draws on public statements, official reports, and associational journals rather than on personal documentation such as correspondence and diaries. To his credit, he has included evidence drawn not only from prominent leaders but also from middle-echelon spokespersons.

CLARKE A. CHAMBERS
University of Minnesota

ROBERT F. WESSER. *A Response to Progressivism: The Democratic Party and New York Politics, 1902–1918*. New York: New York University Press. 1986. Pp. xiii, 328. $35.00.

For many years historians have debated the relationship between Progressivism and the welfare state of the New Deal. Some scholars, notably J. Joseph Huthmacher and John D. Buenker, have argued that the roots of welfare state liberalism run deep into the Progressive era. Others, particularly Otis L. Graham, Jr., have pointed to the reaction of many old Progressives against New Deal "class legislation." Robert F. Wesser's study of Progressivism in New York state adds important insights and detail to the debate. Certain New York Progressives not only accommodated themselves to the New Deal but held central roles in its creation. Franklin D. Roosevelt, after all, emerged from New York Progressivism. Robert F. Wagner and Frances Perkins reflected other sides of the enormously complex, protean Progressive movement.

Wesser is not new to this terrain. His *Charles Evans Hughes: Politics and Reform in New York, 1905–1910* appeared in 1967. Drawing on an array of manuscript and other sources, Wesser's current book follows the New York Democratic party from the turn of the century through World War I. Major events and public figures march past in quick succession (so quick, indeed, that one wishes this had been a longer volume). By the end, Wesser has created a focused picture of political developments in New York during the Progressive era.

At the center of the picture lies Tammany Hall. The machine found itself under perennial assault in these years by good-government Democratic reformers. Practical politician that he was, Boss Charles Murphy responded in part by advancing the careers of two "clean" young Tammanyites, Al Smith and Wagner. All three men became involved in promoting an urban, social welfare agenda. Because the Progressive party died in New York after 1912–13 and the state Republican party grew increasingly conservative, Democrats discovered they could benefit by taking the general direction that would become familiar in the New Deal. By 1918 the Democrats of New York had replaced the Republicans as the party of governmentalist innovation, which now primarily meant social welfare and labor measures. At the national level Woodrow Wilson's administration to some extent took the same tack. The administration (including its young assistant secretary of the navy, Roosevelt) abandoned its fitful skirmishing with Murphy and reached an accommodation with Tammany Hall.

By no means had the full agenda for a welfare state yet been developed or adopted by the state or national Democratic party as of 1918, yet reform along these lines had clearly become identified with the Democrats. The political rewards of this urban liberalism started to materialize, too, in the form of ethnic, working-class votes. The fundamental change in policy and political orientation that so powerfully separated the Republican and Democratic parties in the 1930s had already taken place before the 1920s began.

For all the complexity of his study, Wesser has written it with grace. The volume proceeds chronologically with an interpretive concluding chapter. The book should find an important niche in the literature of the Progressive movement. And, with its emphasis not on Democrats alone but on the Republican, Progressive, and Socialist parties as well, it will be of considerable use to students of party history.

JOHN J. BROESAMLE
California State University, Northridge

DAVID O. LEVINE. *The American College and the Culture of Aspiration, 1915–1940*. Ithaca: Cornell University Press. 1986. Pp. 281. $29.95.

In America since the 1960s democracy has been defined to mean the inclusion of the entire population, for instance in meaningful access to higher education. Before World War II educators used the rhetoric of democracy but tacitly intended it only to apply to the white Protestant middle class. David O. Levine's book draws its driving force from this disparity. He applies a perspective of the 1960s to the examination of American colleges of the interwar period. In 1986, is this presentism?

The opening chapters set forth the democratic rhetoric and tendencies of the prewar era, including the rise of the urban university and of the junior

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college. Levine maintains that most students wanted utilitarian career training, which they increasingly received. The liberal arts, identified with elitism, generally declined within individual campuses. Experiments such as that of Robert Hutchins at the University of Chicago were doomed.

Later chapters show that a positive suspicion was directed at the few students who were too serious or intense and toward Jews. A hard-hitting chapter exposes the colleges' frequent anti-Jewish quotas and refusal to admit blacks. This is not fresh scholarship, but it is well presented.

Whether Levine has intended to write a general history of American higher education in the 1915–40 period or only a history of access to it is not always clear. Chapters deal with the collegiate response to World War I and to the Great Depression (during which student radicalism is deemed to have been unimportant). Clearly, he thinks that limitations on access colored the entire institution.

The book may thus reduce intellectual history to social history, which is inappropriate for this topic. The author comes perilously close to implying that social mobility is the whole meaning of life. Moreover, Levine focuses on the undergraduate college, not the research-oriented graduate school. His concern is thus not truly comprehensive. Finally, the optimism with which he records democratic rhetoric and achievements does not always mesh well with his sense of the shortcomings to produce a satisfying unity of tone. The earlier chapters are almost blandly optimistic, and the final ones gloomy with awareness of the power of educators' prejudices, for instance against Jews.

The book has another argument, strongly presented in the opening chapters. It is that in the history of American higher education, 1915–40 was a distinct period, which was more like the following than the preceding period. The university before 1915 is pictured as dim and elitist, and its democratic rhetoric is dismissed as an unimportant harbinger of what occurred after World War I. On the other hand, the GI Bill is minimized as a turning point in American higher education. The great disparities between the 1920s and 1930s are deemphasized to make the span of the book appear natural.

This is a book that has virtues and limitations.

LAURENCE VEYSEY
Lahaina, Hawaii

EDWARD R. KANTOWICZ. *John Cotter: 70 Years of Hardware*. Chicago: Regnery. 1986. Pp. iii, 270. $12.95.

ROBERT LACEY. *Ford: The Men and the Machine*. Boston: Little, Brown. 1986. Pp. xix, 778. $24.95.

Despite the manifest differences in the size and reputation of their subjects, these two books have a certain comparability. Both are business histories of entrepreneurial enterprises. *Ford* deals with the manufacture and sale of a new product in new ways, whereas *John Cotter* examines new means of marketing an old product. Both studies also center on family enterprise—the creation, expansion, and operation of firms by founders and their descendants.

John Cotter is the better book despite its narrow focus—the evolution since 1947 of Cotter and Company, a hardware wholesaler. In many ways the book is a model monograph because, although immersed in an esoteric specialty, the reader can readily appreciate its detail, logic, and significance.

Besides researching thoroughly and writing crisply, Edward R. Kantowicz provides a sound historical framework and asks perceptive questions. He reviews the rise of mass retailing to document the threat to individual retail hardware stores in the 1940s. He also demonstrates that the ambitious, experienced, and observant John Cotter was a logical candidate to respond with a little-used marketing strategy. His company, a hardware wholesale enterprise owned by the retailers it supplied, was nevertheless conceived, administered, and directed by Cotter and his associates as professional managers. The firm became a national enterprise in the 1960s by organizing high-volume purchase and distribution, which generated competitive costs and national recognition (as True Value stores) for separate hardware retailers. By 1985 Cotter and Company had six thousand nine hundred members, 20 percent of the hardware market, and 1.8 billion dollars in sales.

Kantowicz's account of Cotter's rise also touches on much of the transformation of American retailing in the past forty years—the decline of fair-trade laws, the rise of discount stores, the suburban retailing boom, and franchising. My major criticism is the author's relatively uncritical approach, which downplays Cotter's limitations and the apparently important role of Teamster-organized transportation for his firm. The problem is especially clear in the last two chapters, in which Kantowicz abandons his historical framework to compare Cotter's work to the prescriptions of Thomas Peters and Robert Waterman's *In Search of Excellence* (1982). The author thus understates the book's logical conclusion—the paradox of regimentation by the visible hand of professional management in large enterprise in order to preserve Adam Smith's world of small, owner-operated firms.

Ford is a more complex story but offers less to historians. Robert Lacey writes a familiar account that emphasizes the men rather than the machines. Henry Ford's challenge to the Selden patent and his crowning innovations—the Model T and the assem-

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bly line—are compressed into a single chapter. Other chapters are reserved for the first Henry's sexual peccadilloes, his World War I peace ship, Greenfield Village, and similar eccentricities. Automobiles and their manufacture dominate only ten of the first twenty chapters reaching into the 1930s. Lacey's story raises but does not tackle hard questions. How does one account for Henry Ford's odyssey from tinkerer and racer to successful manufacturer and, ultimately, to rigid tyrant and reluctant imitator? How could such a loud, unlovely crank maintain his enormous popularity until death?

Ford might still have been a useful book. The author's focus on prominent individuals and major car models identifies key elements of the company's continuity and culture. A solid, popular history could have updated the quarter-century since the last volume by Allan Nevins and Frank Hill. Unfortunately, Lacey's attention to personality and sensation in Henry Ford II's era crowds out the recent operation of the company, which sets this rich family apart from so many others. We still need a sound current history of Ford.

CHARLES W. CHEAPE
Loyola College
Baltimore, Maryland

SALLY ZANJANI and GUY LOUIS ROCHA. *The Ignoble Conspiracy: Radicalism on Trial in Nevada.* (Nevada Studies in History and Political Science, number 21.) Reno: University of Nevada Press. 1986. Pp. xv, 209. $16.50.

Labor and social historians will find the theme of this volume (the misuse of state power to crush radicals and their organizations) all too familiar. Sally Zanjani and Guy Louis Rocha have used the Morrie R. Preston and Joseph W. Smith case of 1907 to demonstrate anew the perils of being a radical in local unions of the Industrial Workers of the World (IWW) and the Western Federation of Miners (WFM) during the early decades of the twentieth century. Their account of the deceit and injustice associated with the trial of Preston and Smith for the murder of John Silva in Goldfield, Nevada, also serves as a reminder of the unscrupulous linkage that too often existed then between organized employers and public officials and that frequently produced a misuse of state power against labor and its friends.

This thoroughly researched and readable volume examines in great detail the events surrounding the Preston-Smith trial, whose outcome had unusual implications for radical labor and the national socialist movement. Within a year following the death of Silva in a picketing dispute, the powerful IWW-WFM local in Goldfield had been all but destroyed, its membership dropping from over three thousand to a token several hundred. The authors present compelling evidence that labor's enemies conspired to use the Preston-Smith trial to attack and destroy the flourishing radicalism found in the Goldfield locals of the IWW and the WFM. Their analysis of this "ignoble conspiracy" to pervert the Nevada judicial and penal systems—a conspiracy that involved prominent mineowners, two governors, state judges, a newspaper publisher, and an assortment of perjurers—is exhaustive. They conclusively establish the linkage between these conservatives who conspired to wreck the lives of Preston and Smith and to destroy their organizations.

Although overshadowed by the more highly publicized trial of William D. Haywood and others for the murder of former Governor Frank Steunenberg of Idaho, the Preston-Smith case was nevertheless unusually significant to the later history of radical labor. This first full-length, excellent study of the case, which should greatly interest labor, social, and legal historians, is an instructive reminder of the past misuse of state power to serve special interests at the expense of individuals and the public good. It adds greatly to our understanding of the historical role of the courts in labor disputes.

GEORGE G. SUGGS, JR.
Southeast Missouri State University

CHRISTINE A. LUNARDINI. *From Equal Suffrage to Equal Rights: Alice Paul and the National Woman's Party, 1910–1928.* (American Social Experience Series, number 5.) New York: New York University Press. 1986. Pp. xx, 230. $35.00.

Christine A. Lunardini has produced a brief, brisk study of Alice Paul and the National Woman's party (NWP) campaign for women's suffrage. (The title is misleading because the book concentrates on the suffrage campaign of 1910–20, and the period 1920–28 is covered in a brief eighteen-page afterthought.) The thesis is clear and vigorously maintained, namely that Paul and the NWP were principally resonsible for the suffrage victory. Their winning strategy was to focus on the federal amendment and to convert President Woodrow Wilson, who was perceived to be the key to the amendment's passage by Congress. She argues that the NWP accomplished these despite sometimes brutal opposition from antisuffragists and the police and the obstructionist, devious resistance of Carrie Chapman Catt and the National American Woman Suffrage Association (NAWSA).

Lunardini believes that the contribution of the NWP has been slighted in the recent historiography

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of suffrage. She admits that this book is a "labor of love," but the result reminds one that "love is blind." She is so blinded by her admiration for the brilliance, daring, perseverance, and sheer grit of Paul and company that she presents a generally uncritical and wholly one-sided picture. Those familiar with the suffrage campaign after 1912 know of the split between the staid, badly led NAWSA and the dynamic, aggressive Congressional Union (soon to become the NWP). In 1916, however, the NAWSA came under the powerful leadership of Catt and suddenly stormed ahead, guided by her "Winning Plan." To give Paul and the NWP the credit, Lunardini has to demote Catt. In this book Catt is jealous, deceitful, hypocritical, and malevolent. Lunardini blames Catt for the split between the Congressional Union and NAWSA, and she quotes Catt as doubting that suffrage would be won in her lifetime. The source of that quote, however, is the oral interviews of Paul held in 1972–73. Paul harbored considerable animosity toward her ancient opponents. Lunardini maintains that giving Catt and the NAWSA credit for the suffrage amendment is an "injustice to the women's movement" (p. 163), which suggests that Catt and NAWSA were outside the movement.

The fundamental problem of this book in understanding the suffrage victory is the same defect of vision that Paul had. Congress had to pass the amendment, and Wilson's influence in Congress on that issue was extremely limited. The president's support of suffrage was significant, but a better case can be made for his being converted by Helen Gardener and Catt of the NAWSA, who were closer and friendlier to him. But he was not the key, Congress was. The NWP did not have a winning plan for converting Congress, especially after the failure of their "holding-the-party-in-power-responsible" campaign of 1916.

The book ignores the winning of suffrage in the states, which was fundamental to the suffrage victory and central to Catt's Winning Plan. Not Wilson's gradual change but the accelerating number of suffrage states made the difference. In 1912 only six states had women's suffrage, by 1917 the number was nineteen, and three more were added in 1918. Not Wilson but NAWSA's grinding state-by-state victories changed congressional minds. Nothing in this book changes the conclusion that Andrew Sinclair reached in *The Better Half* in 1965: "Indeed, nearly every Congressman was a green toad on a green tree and a brown toad on a brown tree." They voted the way their states stood on suffrage, not the way Wilson stood.

Without question or argument, Paul and the Congressional Union reinvigorated the federal amendment approach and focused attention on it. The rest of the claims—that they forced Wilson's conversion and got the suffrage amendment through Congress and ratified by the states—are dubious and debatable. This is a splendid source for the mind and point of view of the NWP, but it is not a good way to understand why Congress changed. This book makes one wish that the Feminist Press would finally publish Jacqueline Van Voris's study of Catt.

J. STANLEY LEMONS
Rhode Island College

MICHAEL E. WELSH. *A Mission in the Desert: The Albuquerque District, 1935–1985*. Albuquerque, N.M.: U.S. Army Corps of Engineers. 1985. Pp. viii, 262.

Stretching from central Colorado south to the Big Bend of Texas, the Albuquerque District of the U.S. Army Corps of Engineers includes the upper Rio Grande valley with its tributaries, the headwaters of the Arkansas and Canadian rivers, and the entire Pecos watershed. Michael E. Welsh weaves an intricate story of dam building and flood control on these streams from 1935, when Congress approved the first corps project in New Mexico, Conchas Dam on the Canadian River. Wartime military construction and engineering receive thorough treatment, but he emphasizes four great dam projects for which the district largely has existed: Conchas, completed in 1939; John Martin, 1949; Abiquiu, 1963; and Cochiti, 1973. Meant as institutional history and part of a series that will cover all districts and geographic divisions of the corps, the book also deeply examines the politics and economies of three western states.

After Welsh tenuously links the activities of the modern corps with nineteenth-century topographic surveys, he starts into the political maze that eventually brought the corps into New Mexico to build the Conchas irrigation reservoir near Tucumcari. A chapter takes the Caddoa Dam on the Arkansas (later named for Colorado Congressman John A. Martin) to near-completion in 1943 before the author discusses district activities in World War II, principally building Los Alamos and the Trinity test site for the atomic bomb program. Two chapters summarize flood control on the middle Rio Grande until 1955.

Welsh is at his best when describing the complex political and social developments involved in building Cochiti and Abiquiu dams. Damming the Rio Chama, the most important tributary of the upper Rio Grande, near the ancient village of Abiquiu gave the city of Albuquerque a new water supply and a storage reservoir for use into the twenty-first century. As for Cochiti, constructing one of the world's largest earth-filled dams on lands of the Cochiti Indian Pueblo engendered baffling cultural dilem-

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mas. Two chapters deal with the Pecos River basin, much of it in Texas, and with flood protection on the Arkansas River in Kansas. Welsh concludes his work with an inchoate discussion of environmentalism and nonriverine "work for others" since 1970.

This is "company history" without pretense to social analysis or balance on controversial subjects, a quality it shares with virtually all titles in the series. Yet regional historians may wish for more technical information on the big dams, and readers with military interests will miss biographical treatment of the main figures, especially the district's first commander, Hans T. Kramer. Although Welsh marshals an impressive bibliography, the over four hundred endnotes mostly cite corps records and state government files. The text includes, however, thirty-three full-page illustrations of dam construction and portraits of General Kramer as captain-engineer.

ANDREW WALLACE
Northern Arizona University

MARTIN CHERNIACK. *The Hawk's Nest Incident: America's Worst Industrial Disaster*. Foreword by PHILIP J. LANDRIGAN and ANTHONY ROBBINS. New Haven: Yale University Press. 1986. Pp. x, 194. $19.95.

In the early 1930s the Union Carbide Corporation, seeking to transport water for a new power plant, contracted for a tunnel to be bored through Gauley Mountain, West Virginia. The result was an engineering marvel and a human disaster. Hundreds of men were locally believed to have died from acute silicosis, a lung disease caused by inhaling rock dust. The facts, however, were hotly contested, and the incident poorly chronicled; as part of a negotiated court deal, the company held all the records. The one historian who attempted to investigate the incident reportedly abandoned the project after receiving death threats.

Martin Cherniack, a physician and occupational health specialist, has accomplished a heroic task of reconstruction using court records, death certificates, journalistic accounts, and the slender findings of a congressional subcommittee. Most of the men working on the tunnel were black migrant laborers from the Carolinas, Georgia, Alabama, and Florida. In the midst of the depression they were eager for jobs paying thirty cents an hour; they slept in company shacks and helped bore through three miles of solid silica rock. As noted by the congressional subcommittee, the company did not provide even minimal protective measures against the well-known hazards of silica dust. According to local sources, the sheriff simply drove men off the land when they became too ill to work; others were buried in unmarked graves through a mass contract with the local undertaker.

Historians may wish for a more finely textured study of the social and political context of the disaster; Cherniack does little, for example, with oral history. Instead, with the epidemiologist's distaste for "anecdote," he concentrates on quantitative sources. In a painstaking, detailed, and convincing analysis, replete with tables and charts, he carefully compares county mortality rates and available statistical data to conclude that, at a conservative estimate, at least seven hundred men lost their lives as a result of working on the tunnel. His account also makes obvious the complicity of many lawyers, physicians, journalists, and state regulatory agencies in covering up the incident and serving the company's interests in a blanket of silence.

Cherniak's account is a very significant contribution to the slender literature on the history of occupational health and is a reminder that Bhopal and Chernobyl are only the most recent in a series of industrial disasters. It demonstrates the special skills and insights that epidemiologists can bring to historical inquiry and should encourage social and labor historians to pay more attention to the hidden and often tragic history of occupational disease.

ELIZABETH FEE
School of Hygiene and Public Health
Johns Hopkins University

JOAN M. CROUSE. *The Homeless Transient in the Great Depression: New York State, 1929–1941*. Albany: State University Press of New York. 1986. Pp. xii, 319.

The Great Depression was the key decade for the establishment of the U.S. federal welfare structure and for federal, and occasionally local, experiments in relief. Joan M. Crouse chooses to take a state perspective, focusing on how New York dealt with one small segment of the population requiring relief—the homeless transient. Transiency, however, is by definition an interstate problem. The federal government became involved through the Federal Transient Program, which lasted from 1933 to 1935. This program forms the focus of the book as Crouse discusses how New York dealt with transients before the federal program, how the federal program functioned in that state, and what happened to transients in New York after the demise of the federal program. Crouse portrays a fairly enlightened state cautiously dealing with an interstate problem, meanwhile hoping it would not be inundated by transients from other states taking advantage of its goodwill. This topic demands a national perspective, if only to determine whether generous welfare programs in certain states attract transients. In spite of its narrow scope Crouse spends consid-

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erable time placing her state material in a national context, and her conclusions are well researched and thoughtful.

The most compelling part of the book is the images of the transients themselves. Crouse effectively enables the poor to speak for themselves by using such documents as autobiographies and "last hope" letters to the Roosevelts along with the usual bureaucratic sources. The transients of Crouse's study usually had an urban background. They went on the road because they saw no chance of finding a job in their home community and were tired of being a burden to their friends and relatives. Most were single men in search of industrial employment. A few were single women, and some transients were families. For welfare purposes, many had lost legal residence in their home communities, and New York's one-year residence requirement excluded them from most programs there. Before the Federal Transient Program, they moved almost daily from city mission to railroad boxcar to hobo jungle. In New York the federal program mainly offered them work camps in rural areas isolated from job opportunities. A chosen few did get some vocational training and help in finding a job. The demise of the federal program meant a resurgence of former conditions. Photographs of transient facilities underscore their crowded, impersonal nature. Crouse's portrait of these industrial transients counterbalances the better-known portrait John Steinbeck drew in *The Grapes of Wrath* of extended rural families fleeing the Dust Bowl for California. Although a historical study of transients that is national in scope is still needed, Crouse makes a significant contribution toward that goal.

JUDITH ANN TROLANDER
University of Minnesota, Duluth

GEORGE G. SUGGS, JR. *Union Busting in the Tri-State: The Oklahoma, Kansas, and Missouri Metal Workers' Strike of 1935*. Norman: University of Oklahoma Press. 1986. Pp. xiv, 282. $22.50.

Even during the heyday of organized labor's resurgence in the 1930s and 1940s, a combination of worker fear, employer ruthlessness, repressive state and local authorities, and chronic labor glut defeated efforts to form substantial, independent unions in the Tri-State (Oklahoma, Kansas, Missouri) lead- and zinc-mining region. Even the exertion of federal power following the passage of the Wagner Act had little long-range effect. The International Union of Mine, Mill, and Smelter Workers (IUMMSW) succeeded in organizing only about one-fourth of the miners and smelter workers even during the hothouse atmosphere of World War II. A "union" financed and controlled by the mine operators and supported by rank and filers who felt that its de facto closed-shop arrangements with employers were their best hope of keeping their jobs played a major role in deflecting the early enthusiasm for independent organization triggered by the passage of the National Industrial Recovery Act in 1933.

George G. Suggs, Jr.'s, monograph chronicles the often violent story of union failure in the Tri-State in the period 1933–48. Using a wide range of company, union, federal, and state and local records, he vividly depicts the desperate conditions in which the miners and their families worked and lived. Sympathetic to the cause of independent democratic unionism, Suggs nonetheless frankly acknowledges the failures of the IUMMSW leadership, the weakness of union support among the mine and smelter workers, and the ecological and economic problems of the region that would have made any unionizing effort problematic. The result is a modest book, which, by focusing on workers who were largely unresponsive to the appeals of unionism, adds usefully to our knowledge of the "underside" of this turbulent period. Suggs reminds us that even during the height of union activism employers usually held the upper hand. Moreover, even a prounion National Labor Relations Board could do little in the face of hostile state and local law enforcement to protect workers' rights.

Much of the book chronicles the activities of the Blue Card Union, an organization supported financially and otherwise by the mine companies. Fearful of the effective blacklist against bona fide union membership and distrustful of IUMMSW representatives who combined militant activism, injudicious calls for strikes, and financial peculation, the Tri-State's mine workers sufficiently backed the secretive and manipulative leaders of the Blue Card Union to enable them to mobilize hundreds of club-wielding pickets in several brutal confrontations with IUMMSW activists in Picher, Oklahoma, and other mining towns in the mid-1930s. When Supreme Court validation of the Wagner Act caused Blue Card Union leaders to seek affiliation with the American Federation of Labor, whose leaders and programs they had been attacking in the most venomous terms, rank-and-file members stolidly went along, seemingly resigned to following their company-backed leaders so as to remain in good standing and thus keep their jobs. The AFL's cynical acceptance of so blatant a company-dominated union adds another sordid chapter to the federation's often ignominious record in the 1930s.

The book contributes to our understanding of the complex mosaic of worker activism in the 1930s. Suggs makes no broad claims but rather seeks to

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chronicle the sorry state of the region's mine and smelter workers. He does little to analyze the social or demographic characteristics that distinguished union supporters from their company union counterparts, as, for example, Daniel Nelson so ably does in his studies of Akron's rubber workers. Although Suggs's emphasis on the ability of state and local police power to undermine the efforts of the NLRB is a useful reminder of the strength of localism in this quintessentially "federal" period of labor relations, his comments on the character and structure of state and local power-wielding bodies are few and incurious. The writing is clear and straightforward, marred only by confusing chronological overlaps, undigested quotations, and—shame on the copyeditor—a penchant for dangling modifiers. The notes are informative and the bibliographical essay is an outstanding guide to literature and sources. The maps are helpful, but the poorly reproduced photographs add little.

ROBERT H. ZIEGER
University of Florida

SARAH NEWMAN SHOUSE. *Hillbilly Realist: Herman Clarence Nixon of Possum Trot.* University: University of Alabama Press. 1986. Pp. vii, 242. $25.95.

Herman Clarence Nixon (1886–1967) was an outstanding professor of history and political science and a persistent southern liberal during the middle third of the twentieth century. Sarah Newman Shouse's biography of Nixon thoroughly explores his contributions as an academician and a liberal advocate.

Shouse's work primarily focuses on Nixon's activities. He grew up in rural Alabama, not far from Possum Trot, studied under the direction of William E. Dodd at the University of Chicago, worked in the archives of the U.S. Peace Commission in Paris during 1919, taught at several southern universities, including Vanderbilt and Tulane, contributed the essay "Whither Southern Economy?" to *I'll Take My Stand* (1930), and helped organize liberal organizations such as the Southern Policy Committee and the Southern Conference for Human Welfare during the late 1930s. Shouse shows that Nixon's liberal activism steadily developed from the time of his graduate education during the Progressive era until he withdrew from leadership of the Southern Conference for Human Welfare in 1939. During the 1940s and 1950s rumors that Nixon sympathized with Communists and integrationists prevented him from securing a tenured appointment as a professor of history. During those years, however, Nixon taught without tenure at Vanderbilt and served as president of the Southern Political Science Association and the Southern Historical Association.

In her introduction and concluding chapter Shouse argues that Nixon was a classic example of the transition from Victorianism to modernism in southern intellectual history. As a Victorian, Nixon remained committed to the traditional values and personal aspirations he associated with rural communities such as Possum Trot. At the same time, modernism characterized his analysis of the twentieth-century South and his support for liberal reform efforts.

Shouse's argument is plausible. Because her study focuses more on Nixon's activities than on his ideas, however, her assessment of Nixon's intellectual leadership is not persuasive. Granted, to assign Nixon to any particular school of thought would be difficult. He claimed no ideological label for himself and used an ambiguous phrase, "the art of making a living," to describe his ultimate objectives. Nevertheless, Nixon's published works, especially *Forty Acres and Steel Mules* (1938) and *Possum Trot: Rural Community, South* (1941) include enough autobiographical information and prescriptions for southern improvement to permit analysis of his intellectual commitments. To support her assessment of Nixon as a "hillbilly modernist" (p. 186), Shouse needs to devote as much care to describing his thought as she devotes to narrating his actions.

As an account of Nixon's activities, this book is excellent. Shouse's narrative of his youth, education, academic career, and liberal activism is lively. The information she presents concerning Nixon's experiences in Paris during 1919, his involvement in the writing of *I'll Take My Stand*, his leadership in the Southern Conference for Human Welfare, and his contributions to southern historiography certainly enriches our knowledge of these events.

GEORGE E. SIMS
Belmont College

DAVID EISENHOWER. *Eisenhower: At War, 1943–1945.* New York: Random House. 1986. Pp. xxvii, 977. $29.95.

This rich, provocative, and important volume defies easy categorization. It is at once a scholarly work, a popular history, an explanation and defense of Eisenhower's decisions as supreme commander, an attempt to portray the war as he experienced it, a reinterpretation of Allied strategy and diplomacy, and a partial analysis of the origins of the cold war. It is also an effort to understand the historical background to Eisenhower's presidency. In this respect it constitutes a massive, thousand-page introduction in a projected three-volume study of that presidency by Ike's grandson David Eisenhower.

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Throughout this extensive text the author focuses on the complex interaction between military and political factors in Eisenhower's wartime decisions. Although he analyzes Ike's well-known emphasis on maintaining Anglo-American relations, he also delves deeply into Soviet-American relations and what he considers the previously understated and incompletely understood impact of the eastern front on the supreme commander's thinking and actions. Events on that front, he argues, exercised a decisive influence on Eisenhower's decisions as well as on overall American policy, the outcome of the war, and the shape of the postwar world.

In the process of weaving these factors and interpretations into his detailed narrative, David Eisenhower illustrates a subtle and impressive understanding of complex politico-military issues and provides the reader with both new insights into the problems General Eisenhower faced and a provocative set of conclusions. All of this is supported by prodigious research that includes unpublished as well as published official documents and diaries, relevant secondary studies, and personal interviews, impressions, and memories.

Although the author's general thesis is sound and well argued, many historians will find his specific conclusions controversial and questionable. Most notable in this regard is his tendency to attribute to the advocates of impractical military operations larger policy motivations as well as clear knowledge that their proposals were not feasible. Sometimes this approach is illuminating. The available evidence, however, suggests that these individuals were not really so prescient and that David Eisenhower has inadvertently grafted some of his historical knowledge and hindsight onto them.

Fully comprehending a work of this size and scope, not easy under the best of circumstances, is made more difficult by a few editing problems. These include overly long chapters, too many and at times misplaced subordinate clauses, and the use of the increasingly popular but extremely frustrating unnumbered endnotes identified only by page number and part of a quote.

Despite such problems, the extent to which David Eisenhower has been able to weave all his sources, subjects, and themes together into a readable yet serious account makes this volume an extraordinary achievement that fully deserves the attention it has so far received. As a scholarly work, it synthesizes much recent literature and offers original analyses and conclusions that should spark considerable historical debate. As a popular history, it should help counteract the recent resurgence of myths about American political naiveté and "give-aways" to the Soviets during World War II.

MARK A. STOLER
University of Vermont

JOHN LAMBERTON HARPER. *America and the Reconstruction of Italy, 1945–1948*. New York: Cambridge University Press. 1986. Pp. x, 213. $29.95.

This is a study mainly of one aspect of American foreign economic policy in Italy during the three years following the end of World War II. John Lamberton Harper focuses on the efforts of policy makers, however confused or divided in their objectives, to shape a program for aid and reconstruction in a former enemy country. Ambiguity often flowed from miscomprehension, for many of these leaders, notably Harry S. Truman and Dean Acheson, knew little about Italy, particularly its internal economic structure. Moreover, since these Americans were more deeply concerned with other problems that led to the cold war, they had no real desire to learn much about Italy. Yet, as one consequence of the great wealth and power of the United States, major responsibility for Italy's rehabilitation was thrust onto American political leaders.

Even though concerned narrowly with this immediate problem of reconstruction, the book also has a broader purpose. It attempts to assess the basis of the postwar political as well as economic relationship between Italy and the United States. Generally, the author handles the economic analysis with greater understanding than he does the political. For example, he slights the influence of Italian-Americans on the shaping of Washington's treatment of Italy. The British had no sympathy for the defeated Italians and urged Washington to pursue a harsh policy toward them. As earlier students of this problem have demonstrated, were it not for the desire of Franklin D. Roosevelt in 1944 and of Truman later for Italian-American votes, the American reaction toward Italy might have been as uncompromising as that of the British.

As the cold war set in, leniency probably would have followed anyway, for in subsequent years Italy became important in American strategic considerations. At this time Truman and his advisers did not hesitate to use pressure to keep Italy in line with American global objectives. They made it clear that if Italians voted power to Communists the United States would exclude Italy from the European Recovery Program, or Marshall Plan. According to conventional analysis, this warning had the desired effect on the Italian electorate. Yet Harper maintains, as a theme, that, from late 1946 onward, "the United States could threaten and cajole, but the fate of American policy remained very much in the hands of the Italians themselves" (p. 106). He asserts that Italians alone made the crucial choices of the day.

Regardless of the importance of the Italian choices and of the ambivalence of Washington, American policy in Italy succeeded in both its eco-

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nomic and political aspects in these years. The Left failed to obtain power, Italy moved toward recovery, and the United States, as Harper properly points out, acquired a compliant ally that has helped Americans maintain control of the Mediterranean to this day.

To specialists this story and its general interpretations are well known, but some of the details dug out of Italian and American archives and other sources are not. For scholars, therefore, the book's value lies in these details, its synthesis, and its usually balanced judgments. Soundly researched, it offers the most thorough treatment of its constricted subject yet to appear in print.

ALEXANDER DECONDE
University of California, Santa Barbara

MARGARET A. BLANCHARD. *Exporting the First Amendment: The Press-Government Crusade of 1945–1952.* (Longman Series in Public Communication.) New York: Longman. 1986. Pp. xiv, 448. $39.95.

This is an account, taken largely from official records, of the effort of American journalists after World War II to extend the American concept of freedom of the press to other parts of the world. The goal of the "crusade" was to enable journalists from all countries to operate freely throughout the world; this freedom would allow journalists to better inform the people of the world and thus further the chances of world peace. Margaret A. Blanchard contends that this effort, which she calls "the free-press crusade," was ill-advised, ineptly pursued, and, in the end, a failure.

The Americans who led this campaign were executives of the wire services or newspaper editors who served on the board of the Associated Press. They were supported by some members of Congress, and, most of the time, by the executive branch, through the State Department. Most of the discussions of freedom of the press took place before agencies of the United Nations or at conferences set up by the UN. These journalists, Blanchard says, were so zealous in their convictions that they could neither compromise nor understand the attitudes of those in other countries and were so accustomed to an adversarial relationship with government officials that they could not successfully cooperate with them.

The Americans were supported, on most points, by the countries of the Western alliance and were opposed, on almost every point, by the Soviet bloc. In a decisive position were the small countries, the "undeveloped" nations, whose representatives wanted to license journalists, to censor the reports of foreign correspondents, to have the right to mandate "corrections" in other countries' newspapers, and to set up a code of ethics and an international court in which journalists could be tried for violations of the code. They asked for U.S. aid to set up their own information agencies to compete with the American wire services.

One of the weak points in the American position was the immigration law that limited the reporting of UN affairs by reporters from Communist countries. Blanchard uses this issue as a reason to end her study in 1952. When the heating up of the cold war resulted in the firing of American newspaper reporters who might be "subversives," the crusade was over.

Perhaps, but it seems an odd place to stop. The issues that were debated in those seven years are still being argued at the UN, as the author acknowledges in an epilogue, and the principal value of this book is that it brings a better understanding of the roots of the campaign by the less-developed countries for the "New World Information Order" in UNESCO.

Blanchard is convincing when she argues that to try to impose American press values on the rest of the world was unrealistic. But I am left with the impression that she is indifferent to or skeptical of the value of a free press. The journalists who tried to "export" those values might have been naive, and they certainly did lose, but they were fighting on the right side. If we regard the extension of democratic government as a desirable national policy, it must be accompanied by the extension of freedom of the press. Even the Reagan administration takes this position, in Nicaragua.

EDWIN R. BAYLEY
University of California, Berkeley

PIERS BRENDON. *Ike: His Life and Times.* New York: Harper and Row. 1986. Pp. xvi, 478. $21.95.

Counterrevisionism on Dwight D. Eisenhower has begun eloquently with this book by the author of several well-received works of British biography. The best single-volume biography in print, the book examines Ike from childhood through West Point and his army career, command in World War II, and the presidency. It is based on exhaustive research in the papers of Eisenhower's administration and other archives as well as published sources and most relevant secondary works. It is gracefully written, although occasional British or archaic usage may bother some American readers. There are few errors, whether typographical or of usage or fact. The most egregious factual error is "George C. Patton."

Piers Brendon finds revisionist historians' versions of "America's untarnished hero" oversimpli-

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fied as well as overly favorable, failing to explain Eisenhower's apparent contradictions: the appearance of stupidity in an obviously bright man, opaque statements from one who could write and speak clearly, sometimes brilliantly, the incisive commander who vacillated. Brendon does not accept the notion of deliberate pretense.

Seeing paradoxes in American culture, Brendon holds that Ike, given his middle-American background but superior mind, inevitably grew up as a "conventional individualist" well suited to a peacetime army career. The "steely independence" was concealed by a "protective coloring of good fellowship." Here, Brendon finds the origins of Ike's later behavior as commander and president: able to grow in a difficult job, he was still often overcautious and vacillating. Seeking to avoid not only needless offense but also controversy itself, he would have virtually endless patience with a Bernard Montgomery or a John Foster Dulles, even a Joseph McCarthy, yet would seethe inside.

Brendon grants and illustrates Eisenhower's brilliance in command, in analyzing military problems, and in handling difficult allies. He admits and admires Ike's sure touch with the public and many shrewd political acts. Seeing Ike's need to appease the Republican Right to unify a badly divided party, he misses the price Ike had to pay for admission into the party after years of serving Democrats. Brendon is inclined to push Britain farther into the center than it deserves during Ike's presidency and finds him more favorable to Dulles than much evidence indicates. He also ignores Fred Greenstein's recent discovery of Eisenhower's working against McCarthy.

Brendon is also critical of Ike's vacillation, especially regarding unfit subordinates and civil rights. But he sees in Ike four characteristics that enabled his ultimate success in high command and the presidency: "glowing personal charm and magnetism," which made people want to follow him; "antennae . . . delicately tuned to the mood of the moment," enabling him to forge coalitions; "studious moderation, that instinctive cleaving to the golden mean," making him widely trusted; and "the simple virtues which make Ike appear to possess great moral stature" (p. 186). The result of Brendon's research and perceptiveness is a book that does not destroy revisionism on Eisenhower but usefully corrects it.

ROBERT W. SELLEN
Georgia State University

ROBERT F. BURK. *Dwight D. Eisenhower: Hero and Politician.* (Twayne's Twentieth-Century American Biography Series, number 2.) Boston: Twayne of G. K. Hall. 1986. Pp. xii, 207. $19.95.

Now that Dwight D. Eisenhower, soldier and statesman, has become the subject of a growing number of serious studies, a more nuanced portrait can be drawn of this complex and elusive man. Heavily relying on secondary works, Robert F. Burk has ably presented such a portrait in this brief but thoughtful introduction to Ike.

Burk's Eisenhower appears most vividly and enjoyably in the early years, during which small town and Protestant family interacted to mold him in complex and sometimes unexpected ways, and West Point and army met a determinedly ambitious, yet highly moral and ruggedly individualistic, young man. Eisenhower's athletically inspired admiration for teamwork emerged early, as did his distaste for partisan politics. But the success he craved continually eluded him. Burk effectively conveys Ike's frustration at the succession of obstacles that kept him, despite constant and aggressive efforts, in positions and places he had long outgrown. His rise, when it finally came in World War II, was meteoric but, Burk argues, well deserved.

With the end of war and Eisenhower's entrance into civilian life, this essentially sympathetic portrait grows more critical. Eisenhower's grasp for the presidency, cloaked by the need to feel his nation's call, appears disturbingly self-deceptive and starkly manipulative. And, although Burk agrees with most recent scholars that, contrary to appearances, Eisenhower as president was in charge, he questions the results. Eisenhower, he contends, dealt unsatisfactorily with a gamut of critical issues, especially in foreign affairs, in which he demonstrated a dismaying willingness to risk nuclear disaster to achieve world peace.

For Eisenhower, Burk states at the outset, "the roots of greatness were simple and straightforward . . . But the making of a hero, even an American one, is seldom such a neat and tidy matter" (p. 2). This brief but perceptive biography demonstrates this well.

MARY S. MCAULIFFE
Washington, D.C.

ALAN R. RAUCHER. *Paul G. Hoffman: Architect of Foreign Aid.* Lexington: University Press of Kentucky. 1985. Pp. xi, 208. $22.00.

This is the first scholarly biography of Paul G. Hoffman (1891–1974), remembered chiefly today as the Marshall Plan administrator. Alan R. Raucher supplemented Hoffman's papers in the Truman Library with a wide assortment of primary sources and secondary writings. His book is a solid and useful contribution to recent American history that should be owned by all major libraries.

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Hoffman was a personable, intelligent, decent, hard-working, somewhat bland, and almost entirely unoriginal businessman who became a millionaire at thirty-four by selling cars in burgeoning Los Angeles. He headed the Studebaker Corporation from 1933 to 1948, gaining fame for his humane labor relations policies. Although he was effective as head of the Marshall Plan from 1948 to 1950, a stint as president of the Ford Foundation was marred by controversy. During this period Hoffman spent a great deal of his time working for the presidential nomination of Dwight D. Eisenhower. Hoffman became a personal friend and adviser of the thirty-fourth president and in 1956–57 served as a public delegate with the U.S. mission to the United Nations General Assembly. From 1959 to 1971 he was a top-ranking official of the United Nations Secretariat.

Hoffman was a progressive Republican, a moderate whose defense of trade unions, foreign aid, and civil liberties in the McCarthy era earned the wrath of the far Right. He firmly believed in democracy and capitalism, and he clearly had a positive effect on the automobile industry, the cozy and elite world of tax-exempt foundations, and Eisenhower's administration. A modest man, Hoffman fended off would-be biographers during his lifetime (including this writer). Only now, with this study, can historians get a good look at his lengthy and interesting life.

The book is far too brief and is often dry and lifeless. Scores of people, including Hoffman's son Hallock, should have been interviewed by the author. Almost no one comes to life in these pages, not even Robert M. Hutchins, Henry Luce, Henry Ford II, Anna Rosenberg Hoffman, W. H. Ferry, or William Benton. Still, this is a competent, reliable, and objective study that will no doubt serve as the authoritative biography for many years.

THOMAS C. REEVES
University of Wisconsin, Parkside

MARK G. TOULOUSE. *The Transformation of John Foster Dulles: From Prophet of Realism to Priest of Nationalism.* Macon, Ga.: Mercer University Press. 1985. Pp. xlii, 277. $24.95.

This is a thought-provoking, if not all-encompassing, intellectual biography that analyzes Dulles's diplomatic viewpoint, stressing the period from 1937 to the Korean War. Mark G. Toulouse finds that Presbyterianism played a central role in Dulles's thought and that from this a conception of moral law guided his diplomatic dealings. Ultimately, however, Dulles's notion of America's place vis-à-vis this moral law shifted. Dulles, Toulouse argues, succumbed to the common American failing of letting morality deteriorate into moralism, allowing a quest for "conformity to a universal ideal" to degenerate into a "moralistic confidence (self-righteousness) that Americans already embody that universal ideal" (pp. xx–xxi).

To 1945 Toulouse portrays Dulles, who was then active in the Federal Council of Churches and on its Commission to Study Bases of a Just and Durable Peace, as a "prophet of realism" who recognized the imperfect nature of political systems and their leaders. Reflecting his experiences during and after World War I, Dulles conceded the innate hostility of the nation-state system. He strove for diplomatic accommodation rather than dogmatism and for coexistence rather than the blind pursuit of national self-interest. During World War II he pursued a peace based on justice and international accommodation and saw the United Nations as a vehicle for this.

But Dulles changed after the war. He increasingly identified America's national interests with morality and castigated the godless, materialistic Communist system of the Soviet Union, which he believed had a grand design to overwhelm the free world. Toulouse divides Dulles's postwar transformation into four stages. He first advocated cooperation, then noncooperation, then preventing the spread of Soviet power, and, finally, opposition when North Korea invaded the south. Dulles thus became a "priest of nationalism," an avid cold warrior like so many of his diplomatic colleagues.

Toulouse attributes Dulles's metamorphosis to postwar political circumstances and to his religious perspective. Involved in the postwar meetings of foreign ministers, the United Nations, and other diplomatic duties, Dulles as an official representative of American interests was distressed at the growing Soviet military power and its tightening grip over Eastern Europe. He was alarmed as the postwar meetings disintegrated in disagreement. In this environment his religious disposition led him to conclude that Soviet Communism intended to destroy all religious expression. That left the United States as the defender of the free world. The traditional purposes of the church merged in Dulles's mind with those of the nation.

Toulouse concentrates on Dulles's religious roots to the exclusion of other potentially important influences, such as the Wilsonian internationalism of his earlier years. Furthermore, as the author concedes, others with equally strong religious inclinations, such as Reinhold Niebuhr, did not surrender to this moralism. Toulouse does not adequately investigate why this was the case. Nevertheless, he sets out with the limited purpose of scrutinizing Dulles's religious underpinnings, and in this he succeeds. This well-argued and solidly documented

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work deserves the attention of all interested in the career of Dulles.

T. MICHAEL RUDDY
St. Louis University

MICHAEL R. BESCHLOSS. *Mayday: Eisenhower, Khrushchev, and the U-2 Affair.* New York: Harper and Row. 1986. Pp. xvi, 494. $19.95.

In the final year of Dwight D. Eisenhower's presidency a spy plane of the American Central Intelligence Agency fell near Sverdlovsk, in the heart of the Soviet Union. The consequences of this mishap were profound. The president's dream of a detente was shattered. U.S.-Soviet relations soured.

President Eisenhower had agreed to an East-West summit meeting to begin in Paris on May 16, 1960, and include Chairman Nikita Khrushchev and the leaders of France and Great Britain. Eisenhower was to visit the Soviet Union in June. Hoping to slow arms competition and to ease world tensions, the president was particularly eager to achieve a nuclear test-ban treaty.

Two weeks before the scheduled summit meeting, Francis Gary Powers flew a U-2 spy plane from West Pakistan and was downed in the Soviet Union. Unexpectedly, and despite the CIA's contrary assurance to the president, Powers and most of his espionage equipment were recovered by the Russians. This event led to the collapse of the Paris conference and caused Eisenhower to suspend all instrusion into the air space of Communist nations.

In an account of these events, filled with fascinating details about personalities, policies, and intelligence operations, Michael R. Beschloss tells the story of the evolution of the U-2 program. Capable of flying at record-breaking altitudes for great distances and equipped with cameras able to record precise details on the ground, the U-2 was created out of American fear of a Soviet surprise attack. By 1955 the United States was capable of breaching the Iron Curtain and bringing the entire Soviet bloc under aerial surveillance.

The downing of the U-2 has remained a mystery. What caused the plane to fall? Why was the plane permitted to fly at a time of sensitive diplomatic negotiations? What were Powers's instructions in the event of a mishap? Why was Powers decorated as a hero on his return by the CIA? Beschloss does not provide definitive answers to all these questions. But his book contains brilliantly revealing interpretations of these events and of the complexities of policy making on foreign affairs and intelligence.

More than simply an account of the U-2 incident, this book gives new insight into U.S.-Soviet relations. It demonstrates the importance of the personalities of Eisenhower and Khrushchev on the unfolding of events and shows the influence of domestic internal politics on the foreign-policy behavior of the superpowers. Further, it illustrates that diplomacy can suffer when competing with the higher priorities of intelligence agencies and reveals the dead hand of secrecy on efficient, coherent decision making. Finally, the book shows that a popular president can fail to attain cherished foreign policy objectives by an inability to mobilize bureaucracies—diplomatic, military, and intelligence—in support of these goals.

Most important, this book is about the tragedy of mutual misperceptions that drives the cold war. Both the United States and the Soviet Union continue to construct the road to Armageddon out of mutual distrust and fear. In the author's words, "The U-2 was a flaming javelin unwittingly thrown into the dry forest of suspicions and misperceptions that surrounded American-Soviet relations in May 1960" (p. 381). The consequence was that both the American and Soviet governments killed the hope for detente in 1960.

This carefully researched and finely crafted book is a significant addition to our understanding of American diplomatic history and decision making on foreign affairs in a crucial period.

HARRY HOWE RANSOM
Vanderbilt University

PAUL K. CONKIN. *Big Daddy from the Pedernales: Lyndon Baines Johnson.* (Twayne's Twentieth-Century American Biography Series, number 1.) Boston: Twayne of G. K. Hall. 1986. Pp. xii, 324. $24.95.

Books should be judged by neither their covers nor their titles. The present work is a case in point. The title will likely suggest to some a popular account of Lyndon Johnson's life, but in fact Paul K. Conkin's book is not only a serious and scholarly study but also the best biography of Johnson currently available. Conkin is a distinguished intellectual historian, and Johnson has never been regarded as an intellectual. But Conkin, as he states in his preface, makes no attempt to write an intellectual biography. He offers instead a straightforward and trenchant narrative sprinkled with shrewd, though generally cautious, judgments. Although relatively brief (296 pages of text), this lucidly written study is packed with information.

As Conkin emphasizes in his preface, both the Texas hill country and Johnson's extended family were especially strong influences on his life. His geographic roots provided a sense of identity and a source of strength but also contributed to his deep insecurities. The emotional support and intimacy of his extended family, which he enlarged throughout his career to include those whom he worked for or who worked for him, were essential for Johnson,

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and "sentiments and loyalties, not rules and principles guided his life" (p. xi). Ideologically, Conkin places Johnson "somewhere in the middle, yet always willing to move right or left to accommodate a constituency or to get something passed" (p. 127). As for Johnson's actions in Vietnam, Conkin argues that, although John F. Kennedy's foreign policy advisers were important in Johnson's administration, he accepted their advice because "he shared their perspectives" (p. 176). Conkin is especially favorable to Lady Bird Johnson; he credits her with keen intelligence and excellent cultural tastes. Always a valuable political adviser to her husband, she was an active and energetic First Lady.

Conkin gives balanced attention to Johnson's entire career; little more than a third of the book focuses on Johnson as chief executive. Thankfully, Conkin avoids some of the pitfalls found in many recent biographies of Johnson. He refuses, for example, to engage in psychoanalytical speculations, and he is skeptical of a great many of the unsubstantiated "facts" Robert Caro wooed from Lyndon's brother, Sam Houston Johnson, and other interviewees. Conkin applauds Caro for accurately recording Johnson's chronology but questions the validity of many of the more scintillating accounts by Caro and by Ronnie Dugger in their biographies of Johnson. Conkin's book deserves to be read by both historians and the general public. It is a winner.

JIM F. HEATH
Portland State University

RICHARD C. CAMPANY, JR. *Turkey and the United States: The Arms Embargo Period.* New York: Praeger. 1986. Pp. viii, 146. $35.00.

In spite of his book's somewhat misleading title, Richard C. Campany, Jr., eschews detailed coverage of the U.S. arms embargo of Turkey from 1975 to 1978 and the events that led up to it. Rather, he clearly and succinctly explores the extent to which the Turkish case illustrates the difficulty of using an embargo as an instrument of foreign policy. His conclusion is that U.S. policy was both inappropriate and ineffective if it was intended to modify Turkish behavior or to resolve the Cyprus crisis on grounds favorable to the central government of Cyprus.

The embargo, of course, had a domestic side. It expressed the view of Congress that U.S. laws should be enforced. This moral position has been tarnished, however, by selective application of U.S. laws to other countries, such as Israel. The question of other operative factors is also raised. Although Campany's dismissal of the importance of the Greek lobby is unconvincing, he is on firmer ground when he underscores the importance of the legislative-executive struggle over U.S. policy toward Vietnam, which, together with Watergate, was instrumental in leading Congress to reassert control over the executive on foreign policy. Congressional action, he asserts, occurred at a cost to U.S. interests in Turkey—a loss of Turkish trust in the United States, a worsening of U.S. relations with Turkey, a delay in modernization of Turkey's armed forces, and a gap in the military integrity of NATO.

Campany presents a brief, descriptive, textbook-like survey of the political science literature on embargoes and then examines the necessary background on Turkish foreign policy and its domestic components so that Turkey's reaction to the embargo can be better understood. His analysis breaks no new ground, but it is generally well informed, and his judgments, although deserving of greater explication, are generally fair. He is also on target in his discussion of the insensitivity of both Turkey and the United States to the historical considerations that affected their respective attitudes. He believes that, although embargoes are of doubtful value, the practice is likely to continue for domestic political reasons. His discussion of the Turkish case provides a useful introduction to the problem.

BRUCE R. KUNIHOLM
Duke University

MARY FRANCES BERRY. *Why ERA Failed: Politics, Women's Rights, and the Amending Process of the Constitution.* (Everywoman: Studies in History, Literature, and Culture.) Bloomington: Indiana University Press. 1986. Pp. ix, 147. $17.95.

Scholars and political analysts have begun exploring the failure to ratify the Equal Rights Amendment (ERA). The contours of gender and cultural conflict, the strategies of supporters and opponents, the impact of conservative tides on social movements are being described and evaluated. Mary Frances Berry examines all these features within a broad historical analysis of the process by which Americans amend their constitution.

Briefly but incisively, Berry identifies the essential condition—broad and deep consensus—that must suffuse an issue if it is to create the sense of necessity to gain a two-thirds majority in Congress and then approval in three-quarters of the states, since each institution functions as a separate and often idiosyncratic unit. The process is lengthy, complex, and difficult. But it is possible, as Berry proves in her discussion of the three amendments ratified during Reconstruction to enhance the status of blacks and in her more detailed description of the campaigns for the income tax, Prohibition, and women's suffrage amendments that succeeded during the Progressive period. As counterpoint and as an essential but neglected lesson for proponents of the ERA,

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Berry also examines the failed Child Labor Amendment. To each and to the history of the ERA she applies her formula for success and explanation for failure: timing to coincide with periods of reform, educating and publicizing the necessity for constitutional change, evaluating the vagaries of legislative and judicial action, building consensus state by state before the push for federal action.

Berry believes that the ERA is essential to guarantee legal equality but that the mandatory support demands a rationale that the amendment does not threaten men and women who fear assaults on traditional gender roles and family patterns. This short, enlightening brief for desired constitutional change combined with the demands of our federal system contains the best historically grounded advice feminists could want. Its conditions for successfully negotiating the amending process will give feminists little comfort or rest.

LOIS SCHARF
Case Western Reserve University

CANADA

GORDON T. STEWART. *The Origins of Canadian Politics: A Comparative Approach.* Vancouver: University of British Columbia Press. 1986. Pp. xiii, 129. $8.00.

In this slim, well-written volume Gordon T. Stewart examines the years from 1791 to the 1850s in British North America in order to establish the roots of Canadian political life. He argues that the political structures and patterns of the post-1867 era had their origins in the contentious developments between 1820 and 1860, which, in turn, resulted directly from the Constitutional Act of 1791. The act had rigidly separated the executive and the legislative levels of government and therefore had laid the seeds of increasingly virulent and ultimately violent conflict in Lower and Upper Canada. The act had been given a strong "court" or "statist" orientation by men who had not fully appreciated the links that had been established between the executive and Parliament in the United Kingdom in the early eighteenth century. Stewart contends that the reformers of the Canadas wished not to overturn the "statist" nature of government with its control of patronage but simply to broaden the access to power and thus to control of that patronage. The fierce battles that ensued over "responsible government" did not occur between "court" and "country" parties as in late seventeenth- and early eighteenth-century Britain or late eighteenth- and early nineteenth-century America but rather between two different views of how a strong central government should operate. Stewart concludes that by 1850 Canada had developed a statist orientation in its political culture. He further argues that the role of the governors in the 1830s and 1840s and of such party leaders as John A. Macdonald after 1849 led to the emergence of one-man dominance in party leadership. Similarly, the need to build strong parties in the localities meant that patronage was deliberately used in the interests of party. This, in turn, gave Canada a political stability that it would not have achieved otherwise.

A strength of this volume is its comparative dimension. Historians of Canada rarely glance sideways to compare that country's historical experience with others. Stewart owes much to Louis Hartz and Bernard Bailyn, and his arguments on and insights into elites, linkages, and collaboration have parallels in recent imperial and commonwealth history. By concentrating on high politics and eschewing ideology, this stimulating book offers a fresh interpretation of a much traveled period.

JOHN KENDLE
University of Manitoba

CHRISTOPHER ARMSTRONG and H. V. NELLES. *Monopoly's Moment: The Organization and Regulation of Canadian Utilities, 1830–1940.* (Technology and Urban Growth.) Philadelphia: Temple University Press. 1986. Pp. xvii, 393. $34.95.

In this complex and well-written book Christopher Armstrong and H. V. Nelles make several substantial contributions to North American historiography. This detailed and frequently engaging history of what have become some of Canada's largest economic organizations will be of abiding interest to students of technology, public works, business, cities, urban transportation, political economy, or regulation and public choice. The authors examine the development, organization, and regulation of public and private utilities in such areas as water, gas, street railways, power, and telephones, and this demanding task is completed in not only a Canadian but also an international context.

Deftly moving from Ontario to Quebec to British Columbia, and from Halifax to Winnipeg to Calgary, the authors provide brief but convincing case studies of a wide variety of locales. The operation of utilities was unmistakably regionally distinctive, and this resulted in a diverse utilities sector. Yet the book does not degenerate into "one damn example after another"; throughout, Armstrong and Nelles convincingly explain the complex social, political, economic, and legal forces that influenced the development of a highly integrated utilities sector. In short, no one reviewer can hope to provide a comprehensive review of the range of topics and issues discussed in this book. Thus, I will concentrate on two

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major contributions of particular interest to Americans.

The book is a major contribution to the unfortunately small body of comparative studies of Canada and the United States. Although Armstrong and Nelles have not attempted to write a comparative study, they have, nonetheless, dealt with topics and themes that are still misunderstood on both sides of the border. This volume contradicts the persistent and simplistic view that a "collective orientation" pervades Canada as opposed to the dominance of individualism and private property in the United States. Throughout the study, the authors provide examples of the complex and multifaced nature of the Canadian political economy. This evidence should contribute to overcoming the fact that "historical scholarship in the United States is profoundly uninformed regarding developments in Canada" (p. xiii). Armstrong and Nelles conclude that "Canadians are an unsystematic, unreflective people who eschew doctrinal clarity, perhaps owing to the dangers it entails in a culturally and regionally divided polity. A decentralized federalism and a sharply skewed pattern of economic development weighted heavily toward central Canada have forced political settlements to a host of economic questions. The politicization of regulation at the federal and provincial levels thus gives rise to a variety of outcomes" (p. 327). In short, as befits a huge, democratic, industrialized, and federal nation, Canadians labor under few ideological imperatives in struggling with the developmental, organizational, and regulatory issues related to utilities. Canada, as this volume so clearly illustrates, shares much with the United States, but it has also developed distinct approaches and solutions. Canada is not a northern version of the United States.

A second major contribution of the book relates to the repeated calls by many urban historians for more studies that accept the active role of the urban environment in the city-building process; that is, the "urban as process" prescription that accepts the dynamic interrelationships among environment, behavior, and group experience. Armstrong and Nelles provide ample evidence of the value of this approach. Drawing on a wealth of primary sources and secondary works, the authors detail the specific circumstances in each location that explain the development, organization, and regulation of Canadian utilities, and they base their national generalizations not on assumptions and logic but on empirical evidence. In other words, the Canadian experience with utilities produced an intellectual and political legacy, "a conviction that community property could be managed with equal satisfaction and legitimacy by both private and public agencies" (p. 328). But this legacy arose not out of doctrine but out of a rich variety of local and regional experiences.

The book offers much more, and no reader will be disappointed. For Americans, however, a word of caution is in order. This book can be appreciated only by those with a basic knowledge of Canada's history, geography, and political structure. The brief introductory chapter provides but a glimpse for the uninitiated. Thus, for American historians lacking a nuanced understanding of the Canadian experience, this book is not the place to start. It is, however, a fine example of how the Canadian experience can enrich not only cross-cultural studies generally but, in particular, the often culturally bound and parochial study of the American experience.

ALAN F. J. ARTIBISE
Institute of Urban Studies
University of Winnipeg

S. E. D. SHORTT. *Victorian Lunacy: Richard M. Bucke and the Practice of Late Nineteenth-Century Psychiatry.* (Cambridge History of Medicine.) New York: Cambridge University Press. 1986. Pp. xvi, 207. $29.95.

Richard M. Bucke was born in England in 1837 to a large, well-read pioneering family that settled in Upper Canada a year after his birth. Family ill-fortune sent him wandering the United States as a young man, searching for ways to earn a living. An inheritance and an injury brought him home to study medicine at McGill University in 1858. After graduation he pursued further medical and, less formally, philosophical studies in Europe. After several years of private practice, marked by occasional bad health and poor profits, he took a position with Ontario's lunatic asylum service, a branch of medicine for which he had previously expressed distaste. From 1877 until his death in 1902, he was superintendent of the London Asylum.

Bucke is probably best known in the United States for his friendship with and biography of Walt Whitman. S. E. D. Shortt refers to this friendship in the book and, indeed, features as a frontispiece a comment by Whitman on his reaction to Bucke's wards. Shortt is interested in Bucke not as a literary man but as a lens through which to focus on "the nature and genesis of late nineteenth-century psychiatry" (p. 3). But Bucke's life is an exceptionally good vehicle for this type of study because he was a literary man. He wrote down a good deal of his thoughts in letters to prominent moral and medical thinkers and in books, especially the much reprinted *Cosmic Consciousness* (1901).

Bucke was a doctor dealing with insanity when doctors did not know how to treat that illness. Shortt

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makes clear that Bucke was not an innovative medical thinker or researcher but rather a practitioner faced with managing the daily round of a large institution, who tried to keep up on and critically assess the current thinking in his field. Therein lies his value. He has left a record of a practitioner's reaction to the various theories being explored by members of a profession that felt itself being left behind as surgery was actually beginning to be able to cure and other medical fields to diagnose. Psychiatry found itself stuck in the mire of degeneracy, the theory that the mentally ill were somehow evolving backward to an atavistic state. If this were so, then doctors such as Bucke were not healing physicians but rather warehousemen overseeing the storage of hopeless degenerates. Psychiatrists' escape from this passive role came in the assertion that they could scientifically predict insanity and thus protect society from further disease. Given that the psychiatrists and criminologists so involved viewed insanity as an organic and individual malady unconnected with the stresses that modern industrial society put on vulnerable people, much of the scientific theory is gibberish. Shortt's book is valuable in showing how such gibberish gained credence and how it fit into the thinking of a profession seeking self-justification.

JANICE DICKIN MCGINNIS
Calgary, Alberta

MARTIN L. FRIEDLAND. *The Case of Valentine Shortis: A True Story of Crime and Politics in Canada*. Buffalo, N.Y.: University of Toronto Press. 1986. Pp. xi, 324. $24.95.

Martin L. Friedland has written widely on legal subjects. His first effort at applying his legal skills to the study of historical crimes was the much praised (by lawyers) *The Trials of Israel Lipski* (1984), a study of a murder case of 1887 involving a Jewish immigrant and public anti-Semitism in London. The present book looks at a Canadian murder case that had, the author claims, a significant impact on the politics of the mid-1890s in Canada.

Valentine Shortis was a young Irishman from a prominent family who had come to Valleyfield, Quebec, early in the 1890s. There he worked and associated with "low" elements, and there he committed a violent robbery during which he shot and killed two men and seriously wounded a third. Thus far, this was a standard tale of crime, but Shortis's lawyers argued that he was insane and produced evidence from Ireland to demonstrate that both he and many in his family had a history of insanity. Moreover, Shortis's parents spared no expense to have their son acquitted, including using church connections and possibly even attempting bribery. The jury nonetheless found Shortis guilty, and the judge sentenced him to be hanged. The scene then shifted to Ottawa, where Lady Aberdeen, the wife of the governor-general and an acquaintance of the family, became involved. When the cabinet split evenly on carrying out the execution, Lord Aberdeen decided to commute the sentence to life imprisonment on grounds of insanity.

What makes this extraordinary was that the Shortis case was being discussed in the cabinet at the end of 1895 and the beginning of 1896 in the midst of the collapse of Mackenzie Bowell's Conservative government over the issue of separate schools in Manitoba. Friedland suggests that the Shortis case might have had some impact on these greater events, but scant evidence supports this. Friedland's argument is not helped at all by his contention that "one cannot help but think there may be other connections between Shortis and the [Cabinet] revolt that we will never discover" (p. 181). Nor does any significant evidence demonstrate Friedland's assertion that the case was "a major issue in the election" of 1896, which saw Wilfrid Laurier's Liberals oust the Conservatives. To be sure, some politicians, including Laurier, talked about the case, but, significantly, Valleyfield voted Conservative, which scarcely suggests an electorate consumed with rage at Shortis's commutation.

This struggle to fit the square pegs of evidence into the round holes of supposition marred Friedland's *Lipski*, although it did not detract from the convincing account of judicial practice. In the present book, in which the legal focus is on the law governing insanity and in which much of the research is most impressive, the same problem recurs with a vengeance. At one point, for example, the author speculates on the conversation at a dinner for the counsel at Shortis's trial and cites three subjects on which the talk "would no doubt have centered." Friedland may be correct, but no one knows or can know, and this type of fanciful history, unfortunately far from unique in this book, should make every historian squirm. If this is the way lawyers handle their cases, if this is the law's idea of evidence, then we are all in trouble.

J. L. GRANATSTEIN
York University

CLAUDE BISSELL. *The Imperial Canadian: Vincent Massey in Office*. Buffalo, N.Y.: University of Toronto Press. 1986. Pp. xii, 361. $27.50.

This second and concluding volume of Claude Bissell's biography of Vincent Massey is based on extensive private correspondence and on material at

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the Public Records Office and the Public Archives of Canada; the author also knew Massey personally. In the first volume the emphasis was on Massey as patron of the arts. Here the focus is necessarily on the public figure because it covers the years from 1935 to 1968 when Massey was, in turn, Canadian high commissioner in London, a royal commissioner, and the first Canadian governor-general. The volume is marked by the same elegant prose as the first volume, by the fine balance between private and public activities, and by a discerning sympathy for a complex subject.

As high commissioner, Massey generally enjoyed the social side of diplomacy and reveled in the opportunity to hobnob with aristocracy and royalty. He also fitted easily into the diplomatic circles in London in the 1930s as an appeaser who hoped to "save civilization as we know it" (p. 89). The author frankly discusses Massey's lack of concern for the German Jews; he defends Massey against the allegation of anti-Semitism but provides enough direct documentary evidence to let the reader judge. He finds that during the war Massey was frustrated by Ottawa, which ignored his advice, but found great satisfaction in the service clubs and the convalescent hospital that he and his wife established for Canadian soldiers.

After his return to Canada, Massey made a major contribution to public policy as chairman of the Royal Commission on the Arts, Letters, and Sciences. The cold war had focused attention on the humanities as a bulwark against communism, but at the same time some Canadians feared that American leadership of the Western alliance was a threat to Canadian cultural identity. Massey, with his anglophilia and his patrician view of culture, saw a national need to foster the arts so as to provide a barrier to the mass culture of the United States. The elitism of the Massey Report took institutional form in the federally funded Canada Council. As governor-general Massey played the viceregal role to the hilt, convinced that Old World formalities were a necessary counterbalance to the egalitarian tendencies in North American society.

Massey was an untypical Canadian. He was an aesthete fascinated by pomp and glitter, remote from the imperatives of national politics and from the economic and social problems of most Canadians. The author, however, convincingly argues that Massey, in his own way, was a Canadian nationalist who left his mark on some of the major institutions of that nation. Readers may not share the author's sympathy for his subject, but they will appreciate his sensitivity and frankness and will enjoy his prose. The two volumes on Massey are an outstanding example of the art of biography.

H. BLAIR NEATBY
Carleton University

LATIN AMERICA

JAN PACHOŃSKI and REUEL K. WILSON. *Poland's Caribbean Tragedy: A Study of Polish Legions in the Haitian War of Independence, 1802–1803*. Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1986. Pp. xii, 378. $35.00.

Jean Jacques Dessalines, an insurgent black leader in Haiti's war against French colonial rule, is said to have referred to the Polish troops sent by Napoleon to crush the revolution as "the white negroes of Europe" (p. 213). Dessalines drew this telling analogy because, with the liquidation of the rump Polish state in 1796, the Poles like the Haitians were fighting for the ideals of freedom and independence. The tragic participation of the Second and Third Polish Legions in the Haitian revolution (1791–1804) forms the subject of this fascinating and gripping account. (The Second and Third Legions were among several such units of Polish volunteers recruited for French service during the war of 1793–1815.)

Albeit a collaborative effort, this book is largely the work of Jan Pachoński, a prominent military historian and Poland's foremost authority on the legions. Originally published in Poland in 1979, the volume metamorphosed into the present book thanks to the scholarly and linguistic versatility of Reuel K. Wilson, whose interest in the legions parallels that of his Polish collaborator. The authors diligently trace the grim odyssey of 5,280 legionnaires to Saint Domingue, as Haiti was then called, where the greater part of this force, some 4,000 men, died in Napoleon's doomed attempt to restore French colonial domination and plantation slavery. During their bitter sojourn in Haiti, the Poles witnessed French atrocities against captured black insurgents, which both embittered and disillusioned the legionnaires. Polish resolve to fight for France was further weakened by the legionnaires' ignorance of the local terrain and the guerilla tactics employed by the insurgents, coupled with their own susceptibility to the lethal ravages of malaria and yellow fever. The circuitous and often torturous return of the survivors to France, via Jamaica, Cuba, the United States, the Dominican Republic, and England, is also carefully chronicled.

Regrettably, this work is marred by an incomplete index, misspellings, missing endnote numbers, quotations without citations, and faulty punctuation—all the result of inattentive editing. These irritations aside, the volume is important for several reasons. Pachonski's long-time interest in Poland's Napoleonic legions has resulted in an invaluable reevaluation of earlier Polish scholarship on the Haitian revolution. This is a windfall for Caribbeanists, the

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vast majority of whom neither speak nor read Polish. The authors also make judicious use of extensive archival material in Poland, France, England, and Jamaica. This study also convincingly refutes the climatic argument used to explain the French defeat in Haiti. This view, which ultimately denies any credit to the insurgent black army, holds that the French were destroyed primarily by endemic diseases. Pachonski's exhaustive investigations demonstrate clearly and repeatedly that the French defeat was the result of a combination of elements, one of the most important of which was the bravery, indomitable determination, and military skill of the Haitians.

The focus of the study is a single episode of the violent birth of a free Haiti. It represents, nonetheless, an important and permanent contribution to our understanding of the Haitian revolution.

ROGER NORMAN BUCKLEY
University of Connecticut

ALLAN J. KUETHE. *Cuba, 1753–1815: Crown, Military, and Society.* Knoxville: University of Tennessee Press. 1986. Pp. xiv, 213. $23.95.

Thanks in part to huge economic subsidies from abroad, well-trained Cuban soldiers defended Cuba from aggression and successfully fought overseas to advance the empire's foreign policy. Despite this description's contemporary ring, it springs from Allan J. Kuethe's definitive account of Spanish military reform policies in Cuba after 1762. Kuethe's thorough military history shows the significance of a strategically driven policy to buttress Cuba's political loyalty.

Humiliated by the British capture of fortress Havana in 1762, King Charles III reappraised military policies in the Indies, beginning in Cuba, the key to the American Mediterranean. "The central problem in 1762," Kuethe asserts, "arose from the limited contribution of native Americans to defense" (p. 3). Although Charles III "risked losing everything in the long run by shifting too much power away from the metropolis" (p. 29), "in the face of relentless international pressure" and the hope of providing the king "with the means to wage war effectively" (p. 28), the monarch took the risk of arming the Cubans to defend their land for Spain. He even asked the Cuban elite to finance their service in their king's militia forces.

In exchange for agreeing to finance and staff Cuba's militarization, the Cuban elites benefited far more than elites did elsewhere in Spain's American empire. Unusually free trade and easier importation of slaves enriched Cuba's elite. The granting of noble status and of military rank "recruited the flower of Havana's society," especially the leading sugar planter families, into the new militia regiments (p. 58). Special militia units for free blacks and mulattoes co-opted them, too. By the 1780s the competent and reliable white and black Creole militia defended Cuba while Spain's regular forces, whose bulwark was the Army of Cuba, reconquered Florida from Britain.

Cuba's military reorganization was grafted onto its highly stratified society. Because they feared a replication in Cuba of Haiti's slave uprising, but especially because they had already nationalized decision making, most Cuban elites backed Spain after Napoleon invaded it in 1808. With well-informed references to other colonies, Kuethe's learned and lucid account shows that Cuba was "a unique colony where special military needs and a ready access to the royal administration produced unusual results" (p. 177). He decisively disproves the widely accepted argument that Cuba remained loyal to Spain by force because of the overwhelming Spanish military presence in Cuba.

Two omissions are noteworthy. First, what was the link between military reform and economic growth? Large resources were diverted to the military, but Kuethe explores neither the opportunity costs nor the benefits (military construction and increased imperial "foreign aid") of such allocation of resources for economic growth. Second, Kuethe warns against assuming that the same military reform policies produced similar results under different circumstances, but he does not say why. Although he asserts that "the disciplined militia system worked best in coastal areas . . . where the port cities stood to gain from military spending" (p. xii), Kuethe does not say what else intervened to explain why the Cuban militia, victorious over the British, remained loyal to Spain after 1808, whereas the Buenos Aires militia, also victorious over the British, fought for independence.

JORGE I. DOMÍNGUEZ
Harvard University

PETER TURTON. *José Martí: Architect of Cuba's Freedom.* London: Zed; distributed by Biblio, Totowa, N.J. 1986. Pp. vii, 157. Cloth $29.95, paper $12.50.

At first glance, the current interest in José Martí appears slightly baffling: papers and panels at professional meetings, a recent international conference in London devoted entirely to Martí, scores of scholarly articles, and no less than a half-dozen books, including biographies, anthologies of translated poems and essays, and critical studies. That as recently as twenty-five years ago Martí was all but unknown outside of Cuba and beyond a small group of *aficionados* of things Cuban makes these developments all the more remarkable.

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Among Cubans Martí (1853–95) has long occupied a place of central and undisputed importance. Poet, journalist, and political organizer, Martí devoted virtually his entire adult life to the cause of Cuban independence. He fell in battle and immediately acquired the status of martyr. In death he became larger than life. The hagiography and historiography on Martí merged into something of a separate genre of literature and assumed vast proportions. Cubans of all ideological persuasions found in his life a model of political rectitude to emulate and in the corpus of his writing a political vision to implement. They arrived at substantially similar interpretations of the meaning of Martí and the implications of his work. An uncommon consensus prevailed for decades—until 1959, after which everything changed. Martí did not cease to serve as a standard of morality and a source of legitimacy, but the issue turned on who best represented those values: revolutionaries or counterrevolutionaries.

Much of the literature on Martí after 1959 is in large part a debate about the Cuban revolution. Both defenders and detractors of the Cuban revolution claim Martí as their source of validation. For the defenders the revolution represents a vindication of Martí; for the detractors Martí stands for a repudiation of the revolution. The passion of this debate testifies to the continuing power of Martí, and neither side is willing to surrender Martí to the opposition.

Peter Turton's book is the most recent contribution to the debate. This is a biography that examines the evolution of Martí's political thinking about Cuban independence, Latin America, and the United States and the convergence of all three into a more or less coherent world view. It is also a case for Martí the revolutionary.

Turton is successful in both endeavors. Martí is portrayed as a man of diverse and often conflicting ideas, making a painful if erratic ideological journey toward clarity and unity. Turton is especially effective in establishing the experiential correlates to Martí's intellectual development. Similarly, he draws long-overdue attention to the Krausist roots of Martí's thought and thereby provides a larger philosophical context for the evolution of his thinking.

Scholars of Martí will find little new in Turton's Martí, either by way of information or interpretation. In the debate Turton places Martí squarely at the side of the revolution. "He is, indeed," Turton proclaims early in his study, "the forerunner of the present Cuban revolution" (p. 2). And at a later point: "He appears as the true political forefather of Fidel Castro" (p. 64).

For those unfamiliar with Martí and his work this volume serves as a concise English-language introduction. It provides further corroboration for the proposition that an understanding of the Cuban revolution should properly begin with Martí, and not Marx. This is a good place to make that start.

LOUIS A. PÉREZ, JR.
University of South Florida

LEONOR LUDLOW and CARLOS MARICHAL, editors. *Banca y poder en México, 1800–1925*. (Serie historia.) Mexico City: Grijalbo. 1986. Pp. 427.

Although great strides have been made in the study of Mexico's economic history in the last several decades, the history of credit and banking has received relatively little attention. The volume under review is evidence both of a sensitivity to this need and of the considerable efforts that have been made to remedy it. The book presents the results of a colloquium sponsored by the Seminar on the History of Banking of the National Autonomous University of Mexico in 1984. Participating in the seminar were scholars from a variety of institutions in Mexico and one from the United States.

Of the twelve papers included in the volume, two focus on the late colonial period, when merchant houses and church-related bodies were the principal sources of credit. Clara García Ayluardo examines the credit operations of a resident Basque merchant, Antonio Bassoco, who shifted his emphasis to lending after the commercial reforms of 1788 adversely affected his business. She also analyzes the credit operations of confraternities. Using as a case study the Basque *cofradía* of Aránzazu, she shows how it served as a bank of deposit and source of loans until it was weakened by the enforcement of the Consolidation Decree of 1804. The destructive effects of the decree in the Puebla area, as evidenced by the sharply lowered volume of ecclesiastical loans in comparison with private lending, are persuasively set forth in the article by Francisco Cervantes Bello. Both papers demonstrate the value of the data dispersed in notarial records for economic history.

The role of merchant lenders in independent Mexico before the establishment of the first banks in the 1860s is the subject of three papers. One of them focuses on the contributions of merchants to the growth of Monterrey's economic and political influence beginning in the Reform Era. The other two deal in separate ways with the firms that speculated in the national debt, the much maligned *agiotistas*. The authors, Barbara Tenenbaum and Rosa Meyer, although differing in the assessment of their political role, take a more positive view of their economic contributions than did many of their nineteenth-century contemporaries.

With the exception of the essay by Carlos Marichal, which examines the origin of Mexico's banks and banking system in the light of contemporaneous developments in South America, and of the

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concluding article on postrevolutionary governmental debates on banking policy before the establishment of the Banco de México in 1925, the remaining papers are devoted to the origin and evolution of banks and banking during the Porfirian period. Most impressive is Leonor Ludlow's detailed account of the emergence of the Banco Nacional de México in 1884. Using the archives of the bank, she shows how it evolved through the merger of two recently established banks, one with a preponderance of foreign capital, the other of native capital. The special privileges assigned to this bank—its paper currency had exclusive acceptance at government offices, and it served as the government's fiscal agent—reflected the Porfirian state's philosophy of relying on large private enterprise. As the author points out, these arrangements gave rise to a new financial elite that served as one of the mainstays of the regime together with other elites that benefited from governmental concessions in other sectors of the economy.

The editors are to be commended for putting together a readable and physically attractive volume. Although several of the tabular presentations leave something to be desired, this is more than compensated for by the inclusion of an extensive bibliographic guide to the history of credit and banking in Mexico for the period encompassed by the volume.

ROBERT A. POTASH
University of Massachusetts

THOMAS C. TIRADO. *Alfonso López Pumarejo, el Conciliador: Su contribución a la paz política en Colombia*. Bogotá: Planeta Colombiana. 1986. Pp. 289.

When the leaders of Colombia's Liberal and Conservative parties failed to reach a compromise in 1949, the nation's civilian institutions began to break apart, not to be rebuilt until 1957, when the two parties reached a historic accord by which they shared the nation's top offices. In the interval came a mild military government and the worst years of *La Violencia*, the strife that killed perhaps as many as two hundred thousand Colombians.

Thomas C. Tirado has constructed an elaborate argument to demonstrate that one exemplary man, former Liberal president Alfonso López Pumarejo, bucked all the odds of Colombian history to become the real architect of that historic accord, the Frente Nacional. After drawing on a now largely superseded literature that explains the Colombian violence in cultural and psychological terms—through the aggression, sense of alienation, and frustration of an underdeveloped people—Tirado seeks to demonstrate that the political compromises that have characterized twentieth-century Colombian politics were either ruses of one party to gain hegemony over the other or oligarchic pacts against proletarian impulses. In the 1950s, when the civilian institutions were most threatened and Colombians were suffering the ravages of internal war, López Pumarejo rose to conceive and adroitly maneuver toward a true, novel understanding between the parties, which had little to do with the earlier compromises. In his waning years the former president offered the ideology and the personal example that brought peace to Colombia. It is no accident that the book has been heralded by the Liberal party and that it is introduced by its subject's son, former president Alfonso López Michelsen.

In his effort to exalt his subject Tirado has unfortunately set López Pumarejo's peace-making task as an uphill battle against the forces of Colombian history. When Tirado presents López Pumarejo's victory in broad strokes in a short last chapter and through long quotations from the public positions he took during the mid-1950s, the reader is left wide-eyed, incredulous that so much could have been accomplished with a bit of moral suasion and intellectual argument. At the end of the book the aggression of the Colombian people that marks the first chapter is no more. The problems that beset past compromises have disappeared as well. The author's argument might have been more credible had he sought to accomplish less. Reference to the considerable literature on the violence and the formation of the Frente Nacional that has appeared in the past ten years might also have moderated the radical claims of this book.

HERBERT BRAUN
University of Virginia

ROBERT EDGAR CONRAD. *World of Sorrow: The African Slave Trade to Brazil*. Baton Rouge: Louisiana State University Press. 1986. Pp. 215. $25.00.

Robert Edgar Conrad has made notable contributions to the study of Brazilian slavery. His *Destruction of Brazilian Slavery* (1972) is still the best study in English of abolition; his *Brazilian Slavery* (1977) is a useful annotated bibliography; *Children of God's Fire* (1983) is an excellent collection and translation into English of documents on this topic. In *World of Sorrow* (originally published in Portuguese as *Tumbeiros* [1985]), Conrad addresses aspects of the African slave trade to Brazil. More limited in scope and interpretation than his earlier study of abolition, this volume is in some ways simpler in its arguments and interpretations. It primarily deals with the nineteenth-century slave trade, especially its illegal period from 1830 to 1851. Thus, it covers much the same ground as the earlier books by Alan K. Manchester, Leslie Bethell, and Richard Gra-

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ham, although Conrad provides details not available elsewhere in this useful and well-written volume.

The book is organized in pairs of chapters. The first two provide a survey of slavery and the slave trade in Brazil. Conrad argues that Philip Curtin's widely accepted estimate of 3.5 million Africans brought to Brazil is too low and should be raised to 5 million. Conrad, like others, emphasizes the unbalanced sex ratios, high mortality, and low fertility of Brazilian slaves, and he argues along with Robert Slenes and in contrast to Herbert Klein that mortality rates after 1850 remained so high that the slave population declined regardless of flight or manumission.

The next two chapters outline the well-known stories of increasing British pressure for abolition of the slave trade, first on Portugal and then, after 1822, on independent Brazil, and of the reluctance, recalcitrance, and ingenuity of government officials, ship captains, merchants, and slave owners in circumventing or avoiding restrictions. Conrad argues that Liberal governments in Brazil were never fully committed to ending the trade and that Conservative governments actively opposed such a policy. Even "progressive" politicians such as the proimmigration Senator Nicolau Vergeiro were personally linked to the slave trade. Slavery was simply too deeply ingrained to be ended by foreign pressure, but England was too powerful to ignore. Thus, the solution was official illegality and widespread slaving. It was a classic case of the Brazilian situation of a law that never caught on (*nunca pegou*).

Conrad then examines in two chapters the main players in the script: contraband slave traders and their British and North American collaborators. He offers many examples of their callous disregard of the law and argues that after 1831, when the trade was legally abolished, conditions on the slave ships actually worsened since inspections and restrictions on the slavers ceased. He also demonstrates their adoption of the new technology of steam to the ancient business of slaving. In the last two chapters the author discusses the nether world of the *emancipados*, the legally free Africans who were "liberated" by the government and then employed in conditions quite like slaves, and, finally, the internal slave trade in which some three hundred thousand people were transported between 1850 and 1888 to the newly booming coffee districts in a movement Conrad compares to the transatlantic trade.

Throughout the book Conrad's moral outrage is apparent. Although one shares these feelings, outrage is not enough. Slaves appear here only as objects of oppression, hardly ever as actors. The motives of the principal villains also seem one-dimensional. For example, the curious coincidence of defense of the slave trade with growing nationalism in Brazil is not discussed. Such nationalism may have been self-serving, but authors such as Felipe d'Alencastro have argued that defense of the slave trade against British pressure was a major factor in creating a sense of Brazilian unity. Outrage leads to the acceptance of extreme evidence such as the depositions of William Christie, the English consul who, as Richard Graham has demonstrated, was considered by many contemporaries as an unreliable and not disinterested observer. Conrad's evidence of British merchants anxious to engage and invest in the slave trade raises serious questions about the traditional interpretations of West Indian abolition and the economic reasons for subsequent British abolitionism. These are not discussed. Such questions fall outside the scope of this volume, but they underscore that the slave trade, like slavery itself, must be seen as part of the larger political, social, and economic systems in which it operated.

STUART SCHWARTZ
University of Minnesota

EMILIA VIOTTI DA COSTA. *The Brazilian Empire: Myths and Histories*. Chicago: University of Chicago Press. 1985. Pp. xxv, 287.

There is a refreshing quality to Emilia Viotti da Costa's collected essays, which span six decades of the Brazilian empire's social, economic, and political life between independence (1822) and the collapse of its neocolonial political system in 1889. The product of post-1945 training in Brazil and France, Viotti da Costa produced in eleven years first a major study of the process of abolition (*Da senzala à colônia*) and then a series of thoughtful essays (*Da monarquia à república*). Somewhat reworked and translated, her wide-ranging essays put nineteenth-century Brazil under a critical microscope.

The essays fall into three groups. The first covers the political process of both independence and the empire and includes a long analysis ("Liberalism") of the apparent contradiction between the ideology and practice of liberalism in its Brazilian rather than European context. The contradiction helps explain the perennial rightward drift of reformist elements in Brazil's political class; factions rather than parties competed for control over the social cement of patronage and clientelism.

A second group examines Brazil's economy and society. Producers of export staples on large estates with slave labor, planters pressed the imperial state to curb access to public land and to delay massive European immigration as long as possible; for two decades (1850–70) coffee planters in southcentral Brazil refused to complement slave labor with German, Swiss, and Portuguese immigrants, who rejected slave-like, "submissive," work patterns. "Mas-

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ters and Slaves" synthesizes the argument of *Da senzala à colônia*, that abolitionism in Brazil was not the handiwork of idealists, enlightened slaveholders, and members of the imperial family but the result largely of internal change. Viotti da Costa traces the "relatively peaceful character" of slave emancipation there to planters' belated realization that only European immigrants could solve labor shortages and that immigration required the end of slavery. "Town and Country" elaborates on the underlying theme of the earlier essay on liberalism: Brazil's cities "did not become vehicles for the modernization of the interior" (p. 200); urbanization of the ports failed to modernize the provinces. In contrast to Europe, Viotti da Costa finds in Brazil no tension between the "new entrepreneurial bourgeoisie and the established agrarian aristocracy" (p. 195).

One of the last essays takes issue with Gilberto Freyre's concept of Brazil as a "racial democracy." This myth flourished, according to the author, because social conflict was still masked by patronage and clientelism, which allowed a few blacks to filter up the social ladder. The myth evaporated by the mid-twentieth century, when competition for employment and status replaced patronage: class conflict revealed an enduring pattern of racial prejudice and discrimination in Brazil.

Together the essays are more than an introduction to the Brazilian empire, and savoring their originality requires some acquaintance with Brazil's traditional historiography. They reveal a critical mind searching for the links between ruling classes and those ruled, between elites and masses. Like other Brazilian academic iconoclasts of the 1960s, Viotti da Costa was forced to retire from Brazilian academic life in 1969 because of her critical stance toward past and present. Brazil's loss has been the United States' gain.

STANLEY J. STEIN
Princeton University

SCOTT MAINWARING. *The Catholic Church and Politics in Brazil, 1916–1985*. Stanford: Stanford University Press. 1986. Pp. xv, 328. $37.50.

During the 1970s Brazil's Roman Catholic Church emerged as one of the most innovative and influential in Latin America. Its theologians were in the forefront of liberation theology, its hierarchy had successfully challenged the authoritarian military government, and its grass-roots lay groups ("ecclesiastical base communities") reflected an extraordinary activist surge from the base. In those years the church became the voice of civil society, the rest of which had been repressed into silence. The church also played a crucial role in the gradual liberalization that brought a return to the rule of law and in 1985 the election of the first civilian president since the military coup of 1964.

In this skillfully researched monograph Scott Mainwaring tells this story with admirable lucidity. He has wisely supplemented printed sources with interviews with key actors, both at the top of the hierarchy and at the base. A third of the coverage is devoted to the pre-1964 era, when the Catholic Left took form. Although clearly influenced by European movements such as Catholic Action, the Brazilians adopted an increasingly militant stance on such issues as education, housing, and health. Mainwaring shows that the military government's repression of these groups after 1964 radicalized both the victims and much of the clergy and hierarchy. The remainder of the book analyzes first the emergence of the Popular Church (1964–73), its important role in the reawakening of civilian society (1974–82), and then its declining importance as the opportunity for effective party politics returned after 1982.

The author is especially insightful in explaining why the Brazilian church's evolution was so different from that of other churches that faced military repression, such as the Argentinian and Chilean. The uniqueness of the Brazilian church was a product both of its relative weakness (which allowed more room for lay initiatives from the bottom) and of its leadership's care in not becoming identified with any specific parties or ideologies on the Left. What made the Brazilian church impressive in the 1974–82 period was its ability to combine lay initiative with courage from the hierarchy—above all from Paulo Evaristo Arns, archbishop of São Paulo, Christendom's largest diocese.

A decline in the church's political influence began in the early 1980s. With liberalization the church no longer had a privileged role in speaking for civil society. Furthermore, progressives in the church faced mounting opposition from neoconservative forces from within and from Rome. In the 1970s the church played its most important role in twentieth-century Brazil, and Mainwaring makes clear that the forces behind that role are far from dead.

THOMAS E. SKIDMORE
University of Wisconsin

PETER WINN. *Weavers of Revolution: The Yarur Workers and Chile's Road to Socialism*. New York: Oxford University Press. 1986. Pp. xiv, 328. $19.95.

Since the tragic events of September 11, 1973, destroyed the promise of a peaceful, democratic road to socialism in Chile, scholars and political activists, most of them on the Left, have churned out

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literally hundreds of books and articles purporting to explain what went wrong. The best of this work has focused on the historical trajectory of the Chilean Left, especially the evolving relationship between workers and the Marxist parties, the implications of Chile's system of labor relations, and the forging of workers' political consciousness.

Peter Winn's book does not ignore the historical dimensions of these issues but primarily focuses on the attitudes of workers and leaders involved in the revolutionary process of 1970–73 at Chile's largest textile enterprise, the Yarur mill in Santiago. Principally based on interviews done in 1972 and 1973 with workers and union and party leaders, the book offers rare insight into how these people perceived and participated in the momentous events of those years. These events began with the consolidation of the first leftist-controlled union at Yarur, a family-owned enterprise notorious for its repressive and paternalistic labor relations and its links to the pre-1970 Chilean state. They included the seizure and "socialization" of the factory in 1971—processes, Winn argues, that were decisive turning points in the evolution of the Popular Unity government of Salvador Allende. They ended with the coup of September 1973, when the Chilean armed forces occupied the factory and destroyed the union. Agents of the newly instituted military dictatorship then proceeded to physically eliminate, "disappear," or torture many of the union's leaders and supporters. Finally in tragic symmetry, the new regime returned the factory to its former owners.

Winn interprets this whole process as the product of tensions between "revolution from below" (that is, the creative, revolutionary social potential of rank-and-file workers and their union and party leaders) and "revolution from above" (the hierarchical, overcautious, and ultimately self-defeating strategy of the Popular Unity leadership, especially its president, Salvador Allende). Implicit in this argument is the notion that revolution "from below" is good and that it explains what is positive in the balance sheet of the Allende years. Conversely, revolution "from above" is bad, and it accounts for the failures of Chile's socialist experiment and its tragic denouement. It is clear from the book that much can be made of this argument (see especially Winn's interpretation of the dramatic confrontation between Allende and the workers' representatives over the "socialization" of the factory [pp. 182–88]). Yet, on balance, Winn's evidence and analysis seem to indicate that the events and outcome of the process studied in the book obey a somewhat different and more inclusive logic. In the first place initiatives from the top by governmental and Marxist party and labor leaders seem to go further in explaining the turning points in the story at the plant than do worker intiatives from below. This is true of the consolidation of the union, the seizure and running of the plant, and the relative lack of resistance at the factory to the toppling of Allende's government and the restoration of the status quo ante at the mill. More important, one could argue that, rather than dichotomizing the complex dialectic between worker and governmental initiatives, a more fruitful approach to its terms and movement is to focus on the contradictions, shared by workers and national leaders alike, within the strategy of building socialism through the electoral, legal, and cultural means sanctioned by the half-century of Chilean history in which the Left consolidated itself and finally achieved power.

However one reads the evidence, the book deals with a remarkable story, which Winn has greatly enriched through capturing, preserving, and interpreting the testimony of participants. Because this book so vividly raises the central and contested meanings of the Chilean experiment in building socialism, it will have wide appeal and should be issued in paperback.

CHARLES BERGQUIST
Duke University

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

PAUL E. LOVEJOY, editor. *Africans in Bondage: Studies in Slavery and the Slave Trade.* (Essays in Honor of Philip D. Curtin on the Occasion of the Twenty-fifth Anniversary of African Studies at the University of Wisconsin.) Madison: African Studies Program, University of Wisconsin; distributed by the University of Wisconsin Press. 1986. Pp. 378. $16.00.

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REVIEWS OF BOOKS

TO THE EDITOR:

Walter L. Arnstein's review (*AHR*, 92 [1987]: 123–24) of our book, *The Great Arch*, provides what is in many ways a fair, if necessarily truncated, summary of our arguments, given his evident lack of sympathy for the politics of the book. We do indeed believe capitalism to be "exploitative, patriarchal, and injurious," though that does not entail denying that workers may have benefited from "the private enterprise of others." Our point is rather that it is at least arguable that under different social arrangements such benefits might be more equitably spread, at considerably less cost to the disadvantaged.

But there are several specific points with which we must take issue.

1. We do not "ignore" arguments to the effect that England's present malaise may be due, in part, to insufficient acculturation to capitalist values; indeed, we specifically cite Sir Keith Joseph, as well as Anderson and Nairn, to that effect (p. 230). Our book was concerned with the construction of the English state and ends with the 1880s. The lack of conformity of English capitalism to standard sociological models may well shed light on much of what has happened since. Our concern, however, was to show the inability of the models—Marxist and otherwise—to comprehend either the specific character or the origins of capitalism in England, and the role of state formation in its development. In this context, rejection of crude antitheses of "aristocracy" and "bourgeoisie," and awareness of the unusually commercial orientation of many segments of the landed upper classes themselves (from an early date), is important. So is the singular openness of English institutions of governance like Parliament or the county bench to "new men," by comparison with much of Continental Europe.

2. We do not "utterly ignore the legal powers of unmarried and widowed female property owners." Our summary of the position of women in English law (pp. 36–37) makes quite clear that we are speaking of married women. These were the majority, however. Reigning queens, by contrast, were a minority; and, in the case of Elizabeth, we cite literature which specifically discusses the "problems" of her gender for contemporaries. Finally, we do not believe, and nowhere assert, that subordination of women is confined to bourgeois societies. Our point is rather that in the English case, at least, patriarchy remained an essential dimension of the capitalism at issue and cannot be dismissed as a mere archaic "survival."

3. We do not (we hope) proclaim "truisms of English constitutional history . . . with a breathless air of discovery." In general, we scrupulously acknowledge our sources and make no claim to originality for ideas which are not our own—sources which include such venerable constitutionalists as Maitland. In a work whose argument spans 900 years, some "truisms" will inevitably be repeated, because they are true. It is their role in the argument as a whole which matters. In the case of the 1530s, the point was to bring out the importance of the *revolutionary strengthening of "the state"* which occurred (in our view, following Geoffrey Elton) during that decade vis-à-vis largely Marxist arguments which seek to locate the decisive "break" between supposedly "feudal" and "bourgeois" polities a century later.

4. We try not to be "oblivious to chronology" in our use of such concepts as "the working class," and indeed try frequently to distance ourselves from the overly schematic uses typical of sociology. Thus we discuss the protractedness and difficulty of moving from "service" to "contract" in several places (e.g., pp. 95, 116) and stress the novelty of "the working

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class problem," as seen by contemporaries, from the later eighteenth century (e.g., "the growth of fluidity—mobile property, and masterless, unservile labour," p. 116; or "state formation in this period [1780–1850] . . . faced a very different 'population' to the theorizations of any earlier period," p. 129). Moreover, we stress the typically English—and highly specific—regulation of new relations through old forms, as in the endurance of master/servant legislation governing labor contracts to the 1870s. A real problem in this area, which we hope our whole book brings out, is the inadequacy of conventional hard and fast social classifications, based as they are in inappropriate models of what "capitalism" is and how it supposedly developed. For much of the period we cover, it is as fruitless to search for a "proper" working class as a "proper" bourgeoisie or aristocracy. So Arnstein's criticism here is simply puzzling.

DEREK SAYER
University of Alberta
PHILIP CORRIGAN
Ontario Institute for Studies in Education

PROFESSOR ARNSTEIN REPLIES:

In my 650-word review of *The Great Arch*, I did attempt, as the authors are good enough to observe, to provide "a fair summary" of their arguments at the same time that I found several aspects of their thesis unpersuasive.

1. Thus I suggested that "the authors ignore arguments" that capitalist values triumphed "only in part" in modern British society. The authors respond accurately enough that they quote Sir Keith Joseph to that effect. Indeed they do—in a footnote. Yet, in a text filled with "bourgeois norms," "bourgeois worlds," and "bourgeois conditions," they give no impression that they either have heeded his cautionary note or have felt compelled to try to refute it. Sentences like "The Nation, in short, is the master-symbol which sinews capitalism's cultural revolution, displacing earlier vocabularies of legitimation . . ." (p. 195) do neither.

2. From page 12 on, the authors *do* repeatedly insist that "clearly demarcated gender divisions" are "a constitutive feature of all known capitalist civilizations"—and they scarcely refer to other civilizations. It is noteworthy that, in their response, the authors do not deny my contention that they "utterly ignore the legal powers of unmarried and widowed female property owners." On pages 36–37, they summarize the state of the law in the eighteenth and early nineteenth centuries with respect to *married women only*, even though as many as 25 percent of adult females were not. The rest of the book is filled with references to how "the male English ruling class has legitimated its rule" (p. 195). They are right to insist that "reigning queens were *in* a minority," but in France and Germany they did not exist at all. If it was indeed "specifically in England that modern capitalism was so spectacularly to triumph" (p. 85), may not readers be allowed to draw the syllogistic inference that lands in which capitalism advanced less rapidly were even more staunchly patriarchal? One may be tempted to add that, at the moment, female ayatollahs are also in peculiarly short supply.

3. My contention was not that the authors, in discussing "the Revolution of the 1530s," failed to give credit to Sir Geoffrey Elton. As they note on page 43, "On most points we would side with Elton against his critics." My intent was to suggest that they write and debate so much within the framework of "the new social history" that they seem unaware of the extent to which portions of their thesis fall within a (now oft-derided and little-read) tradition of English constitutional history that long predates Elton.

4. It may well be true that the authors do "distance" themselves in particular instances "from the overly schematic uses typical of sociology," but I found little evidence of that, e.g., on page 115, where they summarize the first decades of the nineteenth century as "the years of what we can without exaggeration call the English terror, when the working class was hammered and machined into acceptable relations." Nowhere in the tightly packed and eminently stimulating chapter in which that sentence occurs is there any reference to the fact that during those same years the total population of England and Wales grew at a more rapid pace than ever before or since—it almost doubled between 1791 and 1831. Nor does there seem to be any acknowledgment of the oft-observed diversity of status, well-being, or even political and religious inclination to be found among the urban wage earners, apprentices, domestic servants, and farm laborers of the day. Somehow, a reference to "the working class" is supposed to clarify everything. Messrs. Corrigan and Sayer have read very widely, and they have tackled a momentous theme in an ambitious and provocative manner. I suspect, however, that of their readers I will not prove to be the only historian who would have been more impressed had they not felt compelled continually to judge and to find wanting—by a utopian socialist criterion that has apparently not as yet been realized anywhere in the world—a millenium of English cultural evolution. By analogy, such readers might well have been gratified if, in a sea of (often insightful) abstractions, they had found room for occasional islands inhabited by flesh-and-blood human beings.

WALTER L. ARNSTEIN
University of Illinois

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TO THE EDITOR:

I must take issue with Edward Wynot's review of Richard Lukas's assessment of Polish-Jewish relations during World War II in *The Forgotten Holocaust: The Poles under German Occupation, 1939–1944* (*AHR*, 92 [1987]: 172–73).

Lukas resurrects old stereotypes and apologies. He argues that the criminal activities and pro-communist postures of "some Jews" were sufficient to explain the anti-Semitism of the Polish population. At the same time, he asserts that the Polish political and military establishments did not pursue anti-Jewish policies and that the many Polish attempts to rescue Jews from the Nazis are largely undocumented.

The truth is more complicated. Generally, the Poles were both anti-German and anti-Semitic. Just before the invasion of Poland and during the first months of the war, in the service of national solidarity, the former feelings submerged the latter for a time; but anti-Semitism surfaced again after the defeat of Poland. The attitudes of the Polish political parties toward the Jews remained unchanged during the German occupation. They ranged from fascist support of the Final Solution to communist campaigns to aid the Jews.

Isaiah Trunk describes how the attitude of the Polish government-in-exile and of the Delegatura, its representatives in Poland, deteriorated under the influence of reactionary and anti-Semitic groups within the underground, particularly after the *rapprochement* of the more moderate Sanacja, the conservative Endecja, and the ultra-right-wing National Radical Camp (ONR) as the Soviets approached the Polish frontier. Ultimately, the Delegatura's refusal to give more than a small quantity of arms to the Jewish Fighting Organization (ZOB) contributed to anti-Polish feelings on the part of the Jewish National Committee and the Jewish Coordinating Commission, and these sentiments were communicated to Jewish political movements and organizations abroad.

Lukas also neglects to mention that, in pre-war Poland, anti-Semitism was not limited only to neo-fascist groups such as the Endecja. It was inherent in the programs of all political parties except for the Polish Socialist Party, the very small Democratic Party, and the even smaller Polish Communist Party. These policies continued unabated during World War II, even in the face of the systematic Nazi annihilation of the Jews.

It is true that, until 1943, the Polish underground was too weak to oppose any mass executions and deportations—of Poles as well as Jews—and that the Final Solution was almost complete by the time the underground was strong enough to take action. It is also true that the Council for the Aid of the Jews, the Zegota (RPZ), organized by the Polish government-in-exile and various Catholic lay groups, arranged for large numbers of Jews to be hidden and cared for. Nevertheless, anti-Semitism tainted even the rhetoric of rescue. For example, in 1942, the RPZ invoked its "Christian duty" to help Jews even though "their fate is a well-deserved punishment inflicted on a nation that since the time of Bar Kokhba has been a parasite on the European body politic." Furthermore, in 1943, after nearly three million Jews had been killed, *Narod*, the underground paper and organ of the party of General Władysław Sikorski, Prime Minister of the Polish government-in-exile, deplored the atrocities but added, "Let us not indulge in false grief for a people that is entirely alien to us." Although the Endecja eventually stopped short of advocating total annihilation in its campaign to rid Poland of the Jews, by 1943 this distinction was academic.

Finally, another aspect of Lukas's book requires correction of perspective: his reference to "the other Holocaust." No one denies that the German occupation of Poland was fraught with atrocity and horror. However, the German goal of making Europe *Judenrein*—the systematic destruction of the entire Jewish population, in order to achieve an almost apocalyptic national salvation—was pursued with a single-mindedness uncharacteristic of their persecution of other groups (Gypsies, homosexuals, politicals, and Slavs). It placed every Jew in Europe, however "Aryan" in appearance or assimilated in lifestyle, under an inexorable sentence of death, even, as Lucy Dawidowicz demonstrates, to the detriment of their military objectives and meager resources.

Wynot is correct in calling for "an objective and balanced treatment of this explosive subject." Lukas, however, has not provided it.

FRANCES GLAZER STERNBERG
Rockland Center for Holocaust Studies, Spring Valley, New York

Professor Wynot declines to respond and states that he stands by his original review.

THE EDITOR

TO THE EDITOR:

In a generally favorable review of my *United States and Italy* (*AHR*, 92 [1987]: 771–72), Professor Bruce Kuklick takes me to task for statements in my conclusion suggesting that the execution of U.S. foreign policy is often inconsistent: specifically, that the United States both supported the establishment of a democratic regime in Italy while it, at times,

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utilized means that helped undermine the legitimacy of the very regime it was supporting.

The paradox that troubles Professor Kuklick is at the center of my argument. In modern states, large and frequently conflicting bureaucracies serving multiple constituencies are hard pressed to create a policy that can pass from a generally agreed upon set of ideological objectives to a totally systematic application. In the case of Italy, the United States utilized traditional and (in generally accepted international usage) legitimate means of assisting its allies within Italy—Marshall Plan aid, psychological gestures of reinforcement, training and equipping police forces, and military assistance—as the primary means of cementing a pro-American democratic regime. However, as the threat of a Communist participation in power increased, the United States simultaneously employed other, highly questionable, and ultimately hard-to-conceal means of aiding its allies. The immediate effect of this type of aid is hard to gauge, but I argue that its medium and long-term effect was to help opponents of the U.S.-backed solution call into question the legitimacy of the forces that the U.S. aided and the state they had created. The Italian Communists repeatedly and successfully exploited these issues from the 1950s through the 1970s.

Historians, I think, would do well to recognize the complex and often contradictory nature of the U.S. foreign policy-making process rather than attempt to force some all-encompassing and quintessentially "academic" explanation on policy formulation and execution. The recent Iran-contra hearings have underlined how ideological and personal conflict within the U.S. government led to the execution of conflicting public and secret policies even within an administration that shared a common world view. This latest in a long series of examples of major contradictions in foreign policy management should give pause to the champions of all-encompassing "schools" of historical interpretation.

JAMES EDWARD MILLER
Washington, D.C.

PROFESSOR KUKLICK REPLIES:

I am sorry if my criticism of Miller's book was unclear. If we ask if American diplomacy was democratic or undemocratic, our answer might be that it was inconsistent. Miller repeats this sort of response in his letter by contrasting what he considers "legitimate" and "questionable" dimensions of policy. I would like to avoid this sort of moralizing rubric. What we want, as historians, is a framework of understanding that is not a direct reflection of contemporary conventional political values. We want to ask questions different from the one that structures Miller's book.

BRUCE KUKLICK
University of Pennsylvania

ARTICLES

TO THE EDITOR:

Professor Michael P. Fitzsimmons, in his article, "Privilege and the Polity in France, 1786–1791" (*AHR*, 92 [1987]: 269–95), is free to express whatever opinions he chooses. But as a historian he should also observe certain canons of criticism. He devotes two pages (274–75) to presenting, and criticizing, my arguments on the role of the Notables in the French "pre-Revolution" (until September 1788). By the author's own admission, he bases his views on one article (a brief one, I might add), only to conclude that "Gruder has recently amended her initial position," merely citing in a footnote two later articles (much longer ones, I might also add). Would it not be in accordance with proper critical methodology for the author to evaluate another historian's work in the light of more extensive rather than more limited writings?

VIVIAN R. GRUDER
Queens College,
City University of New York

PROFESSOR FITZSIMMONS REPLIES:

I treated the articles in question, all of which were published within approximately two years of each other, as a body of work. It seemed to me that the general argument established in the initial article—that the Notables were not defending privileges but were seeking to redistribute political authority in the conduct of public affairs—was elaborated, with some changes, as I noted, in the subsequent articles. My comments were directed toward this overall thesis. Professor Gruder's more extensive writings were, in fact, noted and discussed, as the paragraph ending with note 20 and the following paragraph make clear. Whatever differences of opinion there may be between us on the Notables stem from a full consideration of all of Professor Gruder's published work, which I greatly respect.

MICHAEL P. FITZSIMMONS
Auburn University at Montgomery

ERRATUM

In the review article by John Toews and in the reference to it in "In This Issue" (*AHR*, 92 [1987]: 879–907, iv), the name of Professor William J. Bouwsma was misspelled. We apologize for the error.

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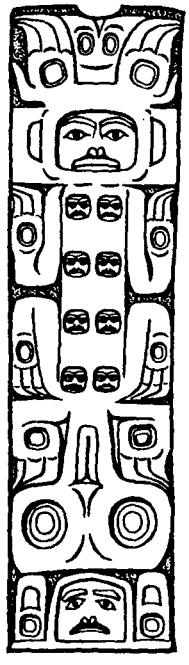
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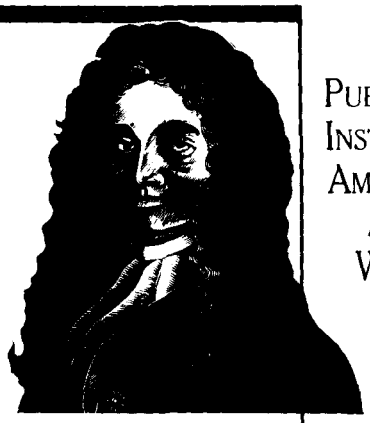
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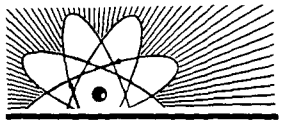
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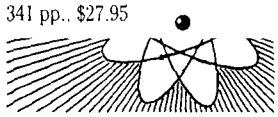
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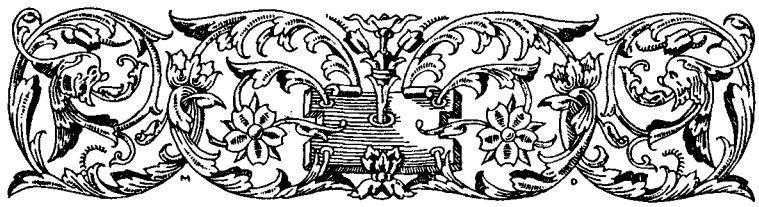
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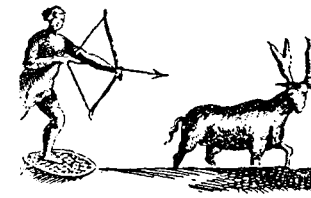
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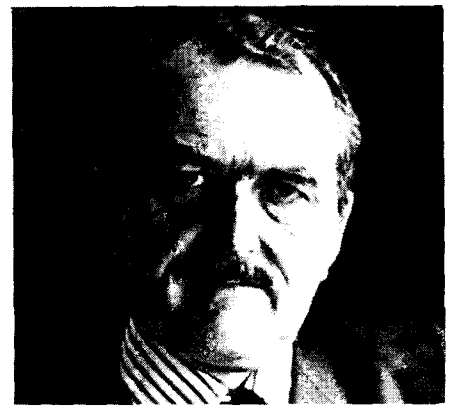
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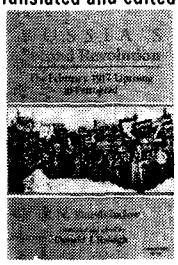
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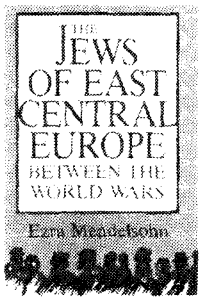
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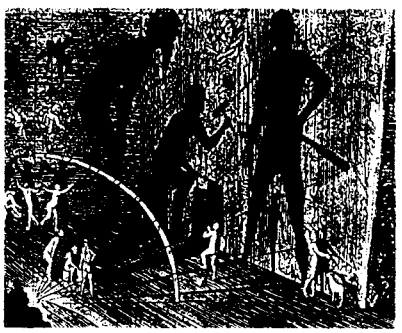
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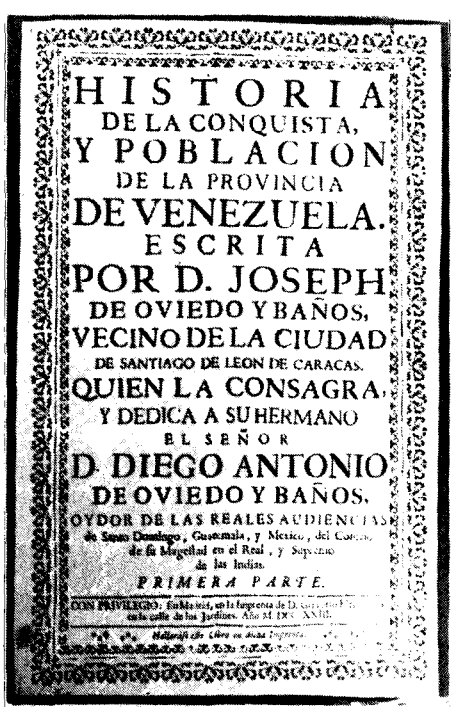
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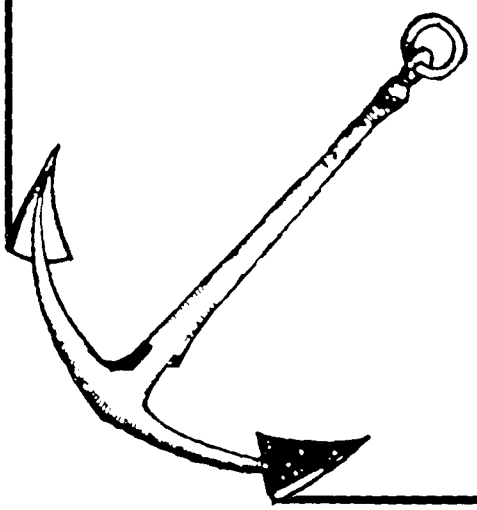
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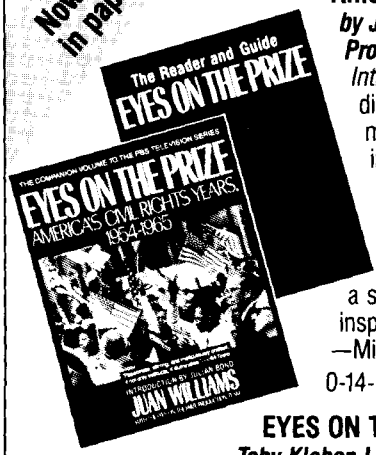
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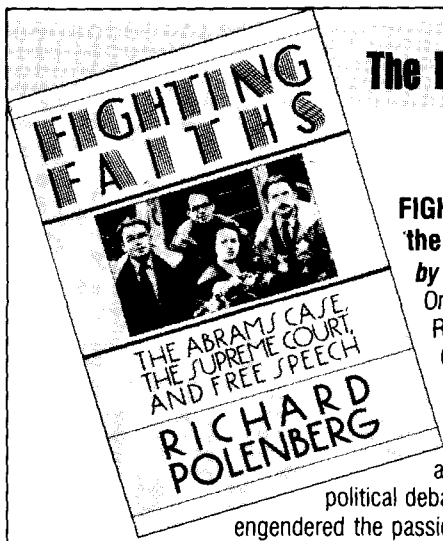
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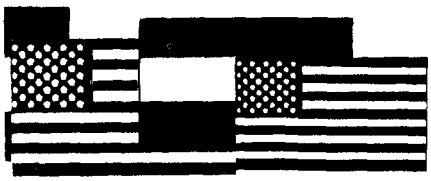
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